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THE

AMERICAN JOURNAL

OF THE MEDICAL SCIENCES

FOR JANUARY 1860.

ART. I.—*Remarks on the Treatment of Inflammation, with special reference to Pneumonia.* By L. M. LAWSON, M. D., Professor of the Theory and Practice of Medicine in the Medical College of Ohio, Cincinnati.

THE history of medicine affords ample evidence that the opinions of physicians, in past ages, were subject to numerous and extensive changes, constituting complete revolutions. The want of a scientific basis enabled the bold or ingenious innovator to supplant the received opinions of the day, and to substitute a new for an old error. But these revolutions are of ancient date. Modern medicine admits of improvement, but not revolutionary changes. In the present, or scientific age, we distinguish between isolated facts, and a connected series of events. The former are accidental and often deceptive; the latter, duly classified and arranged, embrace the entire series of facts and events bearing on the subject, and therefore constitute a science. The shepherd of Melampus could prescribe steel and wine for impotence, or hellebore for phrenzy, without any scientific knowledge of the nature of the disease, or the action of the remedies. And even Hippocrates, by his remarkable powers of observation, was often capable of forming an accurate prognosis, while he possessed but little knowledge of anatomical structures or pathological changes.

In the revolutionary ages, ill-observed facts and crude generalizations too often served as the bases of imposing systems, which reigned supreme until supplanted by others, more specious, but equally false. Thus, the Empirics subverted the doctrines of the Dogmatists, while the Methodists intervened and overturned the theories of their predecessors. And even in comparatively modern times we find the Mathematical school subverting

the Chemical, and the Vitalists superseding the Humoralists. In these speculative systems, a single misinterpreted fact may occupy the place of a science; while obscure technicalities and bold asseverations, are mistaken for rational explanations. The elixir vitæ of Paracelsus, and the antidote of Mithridates, were as probable as the aurum potabile of Bacon, or the similia similibus curantur of Hahnemann; and the Androides of Albertus Magnus was as nearly a scientific revelation as the archæus of Van Helmont or the anima of Stahl. While the cell-evolution of Bennett, although intrinsically true, may, by ingenious misinterpretation, like the debility of Brown, and the spasm of Cullen, become the basis of an erroneous system.

But, within the scientific period—that is, since the successful cultivation of general and pathological anatomy, organic chemistry and general pathology—complete revolutions no longer take place. The attempts to introduce new systems usually result from some sinister cause. The efforts of Brown to subvert the doctrines of Cullen; the visionary and absurd hypothesis of Hahnemann; the crude system of the illiterate Thomson, with its more elaborate but equally empirical fructifications, must be regarded, severally, as the ebullitions of personal envy and jealousy, of dishonest pretensions, and extreme ignorance and presumption. They failed to subvert the regular and progressive course of scientific medicine; nor is it possible, in this age, to inaugurate a revolution. The science may and will be modified and improved, but its leading principles do not admit of sudden and violent subversion.

In opposition to this opinion, however, a recent attempt has been made to destroy some of the leading principles of therapeutics, and which, if carried into effect, would constitute a revolution. I allude to the doctrines recently propagated by Professor John Hughes Bennett, of Edinburgh, seconded, to a certain extent, by Professor Todd, of London, and partly in accordance with the views and practice of the sceptical school of Vienna. The essential features of this doctrine are, that inflammation cannot be properly treated by depletion, the increased flow of blood being a sanative process, but that the powers of the system should be sustained; hence, instead of bloodletting, we should resort to wine and beef-tea! In accordance with this opinion, we find Dr. Bennett treating *pneumonia* with an insignificant amount of salines, together with wine and beef-tea; while Dr. Todd administers brandy in the very commencement of *pericarditis*. Dr. Bennett, however, must be regarded as the propagandist of this doctrine; for while others rely on vague and deceptive statistics, he attempts to reduce the system to definite principles, and thus to place the whole on a scientific basis.

In a work entitled, *Clinical Lectures on the Principles and Practice of Medicine*, issued simultaneously in Edinburgh and New York, Dr. Bennett declares that the diminished employment of bloodletting and other antiphlogistic remedies of late years, does not arise from a change in the

type of disease, as supposed by Drs. Alison, Christison, Watson, and the profession generally, but that the present advanced state of pathology and diagnosis has proved the former treatment to have been erroneous. Hence he concludes that inflammation is the same now it was in former years; and, consequently, that the experience of our predecessors, such as Cullen and Gregory, was based on false views of pathology and errors in diagnosis, and should not be received as guides. And to make the attempted revolution more complete, Dr. Bennett, in a stealthy paragraph, announces that mercury does not promote the absorption of lymph, and is, therefore, in that sense, useless in the inflammatory exudations. Without stopping to argue these questions, I shall introduce his third proposition, which will fairly open up the subject. It is thus stated:—

"*That the principles on which bloodletting and antiphlogistic remedies have hitherto been practised are opposed to a sound pathology.*"

In enumerating the principles on which bloodletting has *hitherto* been practised, Dr. Bennett assumes that it was employed for the following purposes: 1. To diminish the *materies morbi* of the blood; 2. To lessen the quantity of blood which flows *to* the part; 3. To diminish the increased quantity of blood *in* the part; 4. That the character of the pulse was assumed to be an index to the quantity of blood which ought to be drawn. These several points Dr. Bennett proceeds to controvert with great earnestness, and finally reaches the conclusion that practitioners have hitherto been employing their energies in a wrong direction, and that inflammation cannot be relieved by depletion. Let us examine his arguments in the order in which they occur:—

1. "*Can the materies morbi in the blood be diminished by bleeding?*"

This question Dr. Bennett answers emphatically in the negative; and who, let us ask, will assume the affirmative? Certainly no enlightened *modern* practitioner would seriously advocate an opinion which is a mere figment of ancient humoralism, now utterly repudiated by the world of pathologists. Still, Dr. Bennett declares that the "idea of diminishing the morbid matters in the blood has not only descended from Hippocrates to the days of Sydenham, but has come down from his to our own times." It is certainly untrue that physicians of the present day employ bloodletting for the purpose of removing the materies morbi from the blood; and it would appear, indeed, that Dr. Bennett has thus exhumed an obsolete idea for the purpose of giving force to his own untenable positions. So far, indeed, is this charge from the truth that we need not stop to argue its correctness, but merely to brand it as a libel on the intelligence of our profession, and a disingenuous effort to sustain a favourite theory.

2. "*Is it good practice to diminish the flow of blood* TO *the part?*"

This question, asked by Dr. Bennett, involves his main views in regard to the nature of inflammation, and his deductions as to the proper method of treatment. Of course he decides that it is *not* good practice to diminish

the flow of blood to an inflamed part, but that, on the contrary, it is in accordance with sound pathology to augment the determination to the diseased structure, for the purpose of aiding certain ulterior changes connected with cell-growth. The main proposition will be regarded by most practitioners as a novelty in therapeutics, and it will be curious to ascertain how the author can produce even a shadow of reason to sustain an opinion so completely at variance with the received doctrines of the day.

In the first place, Dr. Bennett assumes that the *throbbing* witnessed during inflammation, is a *result* and not a *cause* of the morbid action, and that the blood inducing this phenomenon is not *forced* in by a *vis a tergo*, but is *drawn* in by a *vis a fronte*. And it is further assumed, that this superabundance of blood which is drawn into the part during inflammation, is a special provision of nature to promote cell-growth, whereby the exudation is broken up and rendered capable of being removed, either externally (by suppuration), by passage into the blood (absorption), or by excretion through the emunctories. To accomplish these changes, it is contended that an increased quantity of nourishing fluid is demanded, for example, such as occurs in the ripening of the Graafian vesicles; during the growth of the stag's antlers; in the mammæ during the secretion of milk; in the process of dentition; and during the ascent of sap in plants. Hence, Dr. Bennett declares that as this cell-growth occurs during inflammation, "it is absolutely imperative that the part in which these nutritive changes go on should receive more blood, to enable it to accomplish them." Bleeding, therefore, is an improper agent in inflammation, as it interferes with the nutritive acts necessary to accomplish a cure.

This is a condensed statement of the views entertained by Dr. Bennett, in regard to the nature of inflammation, and the process of cure. It will not be hazarding too much, I think, to assert that the whole statement is at variance with the phenomena of inflammation, no less than the soundest principles of practical experience and observation.

It is asserted by Dr. Bennett that the *throbbing* witnessed during the process of inflammation is produced by the blood being *drawn into* the diseased part, and that this increased flow is a provision of nature to favour cell-growth, and should therefore be encouraged. But unfortunately for this theory, it is all pure assumption, which is not only unsustained by any direct facts, but is actually disproven by certain considerations which I shall proceed to state.

A little attention to what occurs during inflammation will place this question in a proper light, and relieve it of all doubt or misapprehension. The inflammatory process is complex in its character, consisting in part of deranged circulation, which results in a complete *stasis* in the capillary vessels; and, as a consequence of this state of obstruction, the blood is unable to pass through the part, although an increased quantity may be forced into the structures. The result, therefore, is, that the blood being

unable to pass, the arteries adjacent to inflamed tissues become dilated, and *throbbing* ensues. So long as the vessels remain unobstructed, and the blood flows freely through them, the phenomenon of throbbing cannot occur; but the moment the impediment is established, the *vis a tergo* causes the symptom in question. The power, therefore, which induces the throbbing is not a *vis a fronte*, by which the fluid is *drawn into* the part, but is in fact a *vis a tergo*, propelling the blood against an obstruction, the lateral pressure producing dilatation and pulsatory movement called *throbbing*. In making these observations I do not intend to assert that the phrase, "ubi irritatio, ibi affluxus," is untrue; on the contrary, it is evidently a correct observation that an irritation invites an afflux of fluids to the part. But this afflux is a mere initial stage, and induces only swelling and redness, until *obstruction* supervenes, when throbbing becomes a prominent symptom. It is not true, therefore, that the throbbing is evidence of a sanitary process, whereby nature supplies a large flow of blood in order to remove obstructions; but it is a *result* of an impediment to the free passage through the capillaries, and consequently the distension of the vessels adds to the morbid condition by introducing more blood than can be transmitted or consumed in the part.

This explanation, so obviously correct, is entirely at variance with Dr. Bennett's hypothesis. He assumes, it will be remarked, that the throbbing is an evidence of determination to the part, the fluid being *drawn into* the inflamed tissues for the purpose of aiding cell-growth in the transformation of the lymph exudations, and is therefore a sanative process. But instead of blood being *drawn into* the tissues for this or any other purpose, there is in truth complete *stasis* in the part, and consequently no blood can pass through the vessels involved in the morbid action.

Dr. Bennett's view of inflammation is that it begins *outside* the vessels, and that an *active* force draws the bloood *into* the capillaries, and the lymph *through* their walls. All this might be admitted, and yet it would neither sustain his view of the cause of throbbing, nor would it prove the impropriety of bleeding. The first tangible act of the inflammatory process is a determination of blood; and in relation to the present question it is quite immaterial whether the irritation originates within or without the vessels. The ultimate effect, however, is to induce stasis, as before explained, and this gives rise to the *throbbing;* and hence, the latter phenomenon is no evidence of an effort of nature to remove an obstruction by cell-growth, but is simply the sign of an obstruction preventing the free passage of blood.

In attempting to sustain the idea that an increased quantity of blood is a provision of nature to cure the inflammation, Dr. Bennett introduces, as analogous conditions, the increased flow witnessed in the ripening of the Graafian vesicles, in the secretion of milk, etc. These illustrations are peculiarly unfortunate, for they are simply physiological acts, and therefore

cannot be regarded as analogous to the determination of blood connected with a morbid process. In fact these physiological acts do not bear the remotest resemblance to the changes which occur in the capillary vessels during inflammation. In the physiological state, there is simply an increased flow of blood; while in the pathological condition, the vessels become *obstructed*, and morbid *exudation* finally takes place.

If the determination to a part is restrained within physiological limits, then the active circulation, as in the secretion of milk, is beneficial; when the excitement is *morbid*, as in inflammation, there is a superabundance of blood, which, as John Hunter expressed it, is a load on the actions of the circulation, and therefore should be *reduced*. There is too much *excitement*, and too abundant a flow of blood, both of which are abnormal conditions, and should be diminished. But the question arises, can bloodletting diminish this morbid condition?

In the third place Dr. Bennett asks:—

3. "*Can bloodletting diminish the quantity of blood in the inflamed part?*"

This question he answers in the negative. Most assuredly, then, his previous argument, that bloodletting proved injurious by lessening the quantity of blood, and thereby checking cell-action, is wholly gratuitous; but not to take advantage of this evident discrepancy, we will proceed to show the fallacy of this third argument against bleeding.

Dr. Bennett expresses the opinion that the idea of lessening the amount of blood in the inflamed tissues constitutes one of the principal objects in bloodletting, and he denies that such an effect will follow. The chief reasons which he assigns for this opinion are: first, that the blood is stagnated in the inflamed part, and therefore cannot be withdrawn or lessened; second, that if it could diminish the blood in the vessels, it would not assist in removing the exudation which is outside. If the bleeding is large, he argues that it will prostrate the vital powers, and thereby prevent the transformation of the exudations; if the quantity abstracted is small, it cannot produce any effect. He thinks, also, that local bleeding is inefficient, because it cannot be explained on the ground of any direct anastomosis between the vessels of the surface and those of the inflamed part! "From these considerations," he says, "*it follows that neither general nor local bloodletting can possibly be supposed to diminish the amount of blood in internal parts affected with inflammation.*"

It would be difficult to crowd a greater amount of error in any similar number of words. Dr. Bennett appears to have misconceived the whole spirit of depletory practice, and constantly combats monstrous errors, which no sound pathologist for a moment entertains. In the first place, the chief object in abstraction of blood for the relief of inflammation, is not, as Dr. Bennett asserts, designed to withdraw the blood-corpuscles which have already become adherent, nor to remove the lymph which has

exuded outside the vessels; but it is designed *to retard and limit, or even arrest, both the processes of stagnation and exudation.* And this is accomplished by diminishing the quantity of blood sent to the part, by lowering excitement, and thereby checking exudation. The stasis of blood, and the exudation of lymph, are evidently proportioned to the force of the circulation and the degree of inflammatory excitement; indeed, if there is any other law than the degree of excitement directly favouring these actions, I am at a loss to conceive its character. If the excitement and determination of blood are slight, the result will be merely active hyperæmia, or at most a moderate degree of inflammation, and consequently but little stagnation of blood, and a very small amount of exudation; but if the grade of action is high, a large amount of blood will be forced into the diseased part, stasis soon becomes extensive and complete, and the exudation of lymph active and abundant. And who can doubt that the destructive tendencies of inflammation, and the difficulties in securing resolution in ordinary cases, are precisely proportioned to the primary conditions of high action and copious exudation? For example, a moderate grade of pneumonia, limited in extent and intensity, is far less likely to prove fatal, than where a large amount of blood, in the form of active inflammation, is precipitated upon the pulmonary structures, giving rise to a copious effusion of lymph, blocking up the air-cells to an extent incompatible with life.

Now, it is precisely in these conditions of active inflammation that we resort, in the early stage of the disease, to bloodletting and other antiphlogistics, but not for the purpose, as Dr. Bennett states, of removing stasis and lymph, but to prevent or limit these conditions. Take, again, as an illustration, pneumonia, to which Dr. Bennett so often alludes. In the beginning of the disease we find the patient labouring under inflammatory fever, with a full and strong pulse, hurried breathing, cough, etc.; and a physical exploration of the chest reveals the crepitant rhonchus, with the accompanying signs of pulmonary engorgement and effusion of lymph, but short of consolidation. Under these circumstances, it is abundantly established by clinical experience (even by Dr. Bennett himself, as we shall see hereafter), that bloodletting, general or local, together with other antiphlogistics, will moderate the fever, reduce the force of cardiac and arterial action, *and thereby diminish the flow of blood into the inflamed tissues, and proportionally lessen the exudation of lymph;* and hence, if consolidation ensues, it will be comparatively limited in extent, and therefore the more readily resolved.

But Dr. Bennett's arguments apply to an altogether different stage of disease, and an altered degree of excitement. The cell-transformation, for which Dr. Bennett desires so much blood, and on account of which he objects to depletion, is nothing but *resolution*, and does not commence until the force of disease has subsided, and consequently the active stage of inflammation has ceased; hence the period when the vital forces are

required to assist the changes connected with the absorption of the exudations, the organization of tissues or the formation of pus, is not the stage in which any enlightened practitioner, resorts to depletion. On the contrary, we deplete in the stage of *excitement*, and support the strength during the stage of resolution or cell-transformation. Dr. Bennett, therefore, misrepresents the practice on this subject. No judicious physician would think of depletion in the stage of resolution; and no sound pathologist would attempt to increase or sustain the excitement during the forming or active stages of inflammation.

But it may be assumed that bloodletting, even in the early stage, will so far prostrate the system as to prevent the recuperative powers from removing the exudations. To this view I would oppose the argument, that the force of disease, unchecked by antiphlogistics, will tend to more complete and hopeless debility, than arises from judicious depletion; and the difference in the two conditions is, that the prostration from disease alone is of difficult removal, while the debility arising from depletion is more likely to be temporary, and consequently restoration can be the more readily secured by stimulants and nutrients.

The resolution of pneumonia, or the cell-development of Dr. Bennett, is, evidently, a process of *degeneration;* that is, the breaking up of a consolidated lung is a retrograde action, by which the cells liquefy and absorption takes place. Here, as in other examples of inflammation, the two forms of exudation, coagulable and corpuscular lymph exist, and organization or liquefaction, as in other instances, depends on the predominance of one or the other. Mr. Paget observes that the larger the proportion of *fibrin*, the greater the probability of adhesions and indurations; and the larger the proportion of corpuscles, the greater the chances of suppuration or other forms of degeneration. Now, it will hardly be denied, that bloodletting, by lowering vitality, will lessen the amount of fibrinous exudation, and relatively increase the corpuscular, and thus diminish the chances of permanent consolidation. Bleeding, therefore, favours resolution, and hinders adhesions, consolidations, and indurations.

I am not unmindful of the assumed therapeutical law, that bleeding does not immediately reduce the proportion of fibrin. This may be true as an independent proposition; but it is equally obvious, that a lowered vitality diminishes the plasticity of fibrin, and gives the predominance to corpuscular exudation. Hence, bleeding does, in that sense, diminish fibrinous exudation, although it may not be immediately perceived in its proportion in the blood. Bleeding, therefore, becomes appropriate in pneumonia, for the act of resolution is a process of degeneration, and requires a lowered action; indeed, resolution does not commence until the activity of inflammation has ceased.

It may safely be affirmed, therefore, that the determination of blood to a part, during the early stages of inflammation, is not a conservative act,

designed to favour cell-growth; but it is in fact a morbid process, the essential disease itself, ultimately causing the very products which require cell-action for their removal. And it is equally evident that depletion may lessen the quantity, and alter the quality of blood flowing into the inflamed tissues, and thereby limit the exudation, and the tendency to permanent consolidation. Hence the objections to bloodletting apply to the stage of resolution, and not to the early and active periods of the disease. Dr. Bennett's arguments, therefore, are fallacious, and his conclusions unsound.

Dr. Bennett's fourth proposition reads thus:—

4. "*That an inflammation once established cannot be cut short, and that the only end of judicious practice is to conduct it to a favourable termination.*"

This proposition constitutes a fundamental element in the non-bleeding *platform;* but its force is greatly lessened by the fact that even if true, still the violence of the inflammatory process might be greatly mitigated, and a favourable issue secured, although the duration of the morbid action might not be materially abridged. But, passing by this view, let us inquire how far the proposition is intrinsically true, that inflammation must necessarily run its full and complete course.

Dr. Bennett proceeds to show that there are certain forms of disease which cannot be cut short by treatment, but are permitted to run a natural course, among which he mentions typhus fever and smallpox, and adds: "It appears to me that the same rule ought to hold with regard to the internal inflammations," etc. It must certainly be regarded as a very unsound doctrine that simple inflammation is subject to the same pathological laws that govern *specific* disease, and that because the latter run a natural course, no effort should be made to curtail the duration of the former. In support of the opinion that inflammation does not require nor admit of depletory treatment, Dr. Bennett introduces, as analogical conditions, the changes connected with fractured bones, divided tendons, and the resolution which occurs in contusions and lacerations. But how widely different are these processes of repair, which hardly vary from physiological growth, from the morbid action occurring, for example, in pneumonia! In the latter disease, the tissues are filled with blood, copious exudations obstruct the air-cells, and a vital function thus becomes rapidly impaired. Surely there is no analogy, in a true pathological sense, between such a condition, and that which occurs in the repair of a fractured bone, or the union of a tendon after subcutaneous section. It certainly does not follow that, because the surgeon would not bleed in ordinary cases of repair, the physician should equally abstain from depletion in cases of severe visceral inflammation. If, however, we admit the analogy, the difficulty will not be removed; for, as shown by Mr. Paget, *repair* does not commence while there exists a high grade of inflammatory action, and it is only after a subsidence of the morbid state that the physiological process of repair

begins. And so, too, of visceral inflammation; resolution does not begin until inflammation ceases or abates, and hence the necessity of appropriate depletion.

But one of the most palpable errors into which Dr. Bennett has fallen is the supposed similitude between specific disease and simple inflammation. What modern pathologist could for a moment believe that essential fever and local inflammation are governed by the same laws, or that smallpox and pneumonia must equally run a natural course, uninfluenced by medical treatment. It has assuredly been reserved for the great Edinburgh pathologist to utter this profound doctrine, and as an evidence, too, of the present advanced state of pathology. The doctrines of Broussais, of Clutterbuck, and of Pinel, would be no more absurd at the present day, than this idea of a parallelism existing between essential fever and local inflammation. The former is the result of specific causes, which requires a definite and even fixed period for its evolution; the latter may result from general or non-specific causes, and therefore requires no definite or fixed period for its development and decline. Thus, the duration of typhoid fever uniformly presents an average of three weeks, being incapable of reduction below that period; while pneumonia varies in duration from four or five days to quadruple that period. Certainly the similitude is not very striking between specific and non-specific disease, and any opinion based on such an assumption is wholly fallacious.

Still Dr. Bennett insists, as a fundamental proposition, that inflammation once established cannot be cut short; and hence bloodletting—which would not only fail to limit the inflammatory action, but would prove injurious by interfering with the transformations of the exudation—should be abandoned. Dr. Bennett's opinion that inflammation cannot be cut short is evidently based on his peculiar definition of that process, which is, that inflammation consists in "*an exudation of the normal liquor sanguinis.*" Therefore, inflammation is evinced by the exudation of liquor sanguinis, and when this occurs, the process cannot be cut short, but must pass through its regular stages of adhesion, suppuration, granulation, cicatrization, etc. These statements are extremely erroneous, and, indeed, may be regarded as fairly begging the question. In the first place, it is neither an admitted doctrine, nor proven by Dr. Bennett, that inflammation may not exist, in all its essential characters, without the exudation of lymph; and even when exudation has commenced, there is every reason to believe, that judicious depletion may check the effusion, and thereby *shorten* the duration of the disease, which, in a therapeutical sense, is equivalent to cutting it short.

A little attention, however, to what occurs during the forming periods of inflammation, as observed in the web of the frog's foot, and other transparent structures, will serve to place the question in its true light. An irritant applied to the part produces the following effects: 1. Contraction

of the vessels; 2. Dilatation; 3. Irregular movement of the blood; 4. Stagnation; 5. Exudation. Now the question arises: Does not *inflammation* exist until the fifth or last stage?

Without entering into any speculative considerations as to the essential nature of inflammation, it may be safely affirmed that the demonstrable part of the process is *intra-vascular*, and that the exudation into the interstitial tissues is altogether secondary, is variable in character and extent, and may or may not occur. The interstitial changes embrace exudation of lymph, adhesions, formation of vessels, suppuration, and so on; but these conditions are variable in degree and character, and are merely *results* of the primary lesion. Hence, the exudation of lymph is no more an essential part of the inflammatory process, than is the secondary changes of adhesion, suppuration, or gangrene. In illustration of this subject, it may be further remarked, that inflammation is known to occur in certain tissues without the effusion of lymph, as, for example, in cartilages (Goodsir, Redfern), and in the cornea (Virchow). And in addition to this we may add, that inflammation of the mucous and dermoid tissues may often exist with little if any exudation of normal liquor sanguinis. Indeed, the elimination from the surfaces of mucous tissues, in moderate degrees of inflammation, seem merely modified mucus, or, at most, the corpuscular variety of lymph, and therefore cannot with propriety be called *normal liquor sanguinis*. The *essentials*, therefore, of inflammation, in a practical sense, consist in the local excitement, engorgement of the vessels, stasis of blood, and a *tendency* to exudation of normal liquor sanguinis, which latter may or may not occur.

So far, therefore, as pathological conditions serve as therapeutical indications, there is no room to doubt that the essential process of inflammation is, under certain circumstances, amenable to timely and judicious medication, and that it need not, as Dr. Bennett supposes, necessarily pass through all its stages, or run a natural course. And it is quite immaterial whether we adopt or reject the proposed definition of inflammation, for it may be in technical terms correct, and yet the deductions entirely erroneous.

But the ingenious theories based on obscure definitions, and the deductions from hypothetical premises, are most certainly corrected by practical observations and clinical experience; and hence, that which the speculative pathologist attempts to establish by abstract reasoning and forced analogies, the practitioner refutes by daily experience in the treatment of disease. Thus the theorist asserts that inflammation cannot be cut short by treatment, and he proves the assertion by hypothetical conclusions; while the enlightened physician is taught by practical experience that timely depletion will either mitigate the violence, shorten the duration, or arrest the progress of inflammatory action.

Inflammation of the air-passages and pulmonary substance affords some of the most conclusive examples in support of the opinion that phlogistic

disease can be arrested short of its full and complete course. Thus, a child is attacked with fever, cough, hoarseness, and difficult inspiration; this is the forming stage of croup, *prior to the effusion of lymph*, but still a state of inflammation. If such patients are bled, vomited, and purged, the symptoms subside and recovery ensues. On the contrary, if left to nature (or, according to the new theory, *supported*), the violence of morbid action rapidly increases, a false membrane is formed, and the child dies, not exhausted, but asphyxiated. These are the probabilities; and so constant and uniform are the results, that clinical experience has established a scientific rule in practice. And the same events occur in pneumonia, both in children and adults. A world-wide experience demonstrates that this affection becomes fatal in an exact ratio with its duration prior to treatment. Grisolle observed, with the greatest accuracy, a direct relationship between the mortality in pneumonia and the period when treatment was commenced. Thus, in cases admitted within the first three days, the mortality was one in thirteen; within four days, one in eight; within five days, one in six; within six days, one in four; within seven days, one in three; and when eight days had elapsed, one-half perished! Such facts are incontrovertible. They clearly exhibit the influence of treatment (antiphlogistic) over the course of disease, in moderating its violence, and leading to a favourable result. But we need not resort to statistical evidence to establish the influence of treatment either in arresting the progress or mitigating the violence of inflammatory disease; for no amount of speculative pathology or ingenious hypotheses can subvert the clinical experience of the great body of our profession.

In attempting to form correct opinions in relation to the influence of depletion over inflammatory action, it is a very partial and erroneous view to limit our observations to what is taking place in the capillary vessels. Dr. Bennett may be able to see nothing in the inflammatory process except the effusion of lymph, nor to comprehend any therapeutical act save that of cell-transformation as observed in adhesions, suppuration, and gangrene. But the practical pathologist and therapeutist sees in addition the commotion produced in the general system; he does not, with Dr. Bennett, ignore the *inflammatory diathesis;* but he fully appreciates the abnormal excitement of the heart, arteries and nerves, and the relation which this condition sustains to the local disease. He is at the same time fully aware of the changes which are taking place in the capillaries; he knows that the *stasis* will be in proportion to the violence with which blood is not only *drawn* but *forced* into the diseased part, and that the exudation will in turn bear a direct relationship to the degree and extent of stagnation. Furthermore, it is evident that the destructive tendencies of inflammation, either in arresting the functional action of important viscera, or in those secondary changes connected with adhesions, suppuration, gangrene, and so on, will be proportioned to the extent of effusion, not less than the recuperative

powers of the system. Hence the philosophical practitioner seeks to limit the morbid process by lessening the inflammatory diathesis, where the disease is not purely local, and for this purpose he reduces vascular and nervous excitement generally, and either arrests inflammation or limits its violence and destructive tendencies.

It is a most palpable and serious error to attempt to draw indications of treatment from what occurs during the stage of cell-transformation, a period when the essential process of inflammation has ceased, and when brandy and beef-tea might be tolerated or even demanded. It is thus by looking alone to the terminal acts of inflammation—the conservative or destructive part of the process, as the case may be—that Dr. Bennett and his coadjutors have committed the great error of rejecting depletory treatment, and encouraging an indiscriminate stimulation. Whereas, the discriminating practitioner adapts his remedial agents to the constitution of the patient, and the stage and character of the disease; and thus he is often obliged to deplete in one stage, and stimulate in another, thereby protecting the system from the violence of over-action, and sustaining the vital powers during resolution.

The following is Dr. Bennett's fifth and last proposition:—

5. "*That all positive knowledge of the experience of the past, as well as the more recent observations of the present day, alike establish the truth of the preceding principles as guides for the future.*"

Dr. Bennett here reaches the culminating point. Having, as he assumes, established his principles, he deduces the preceding proposition, and remarks that "the more exact observations of the present day" establish these doctrines as guides in practice. What Dr. Bennett means by "the more exact observations of the present day" is, I presume, his own opinions; at least the propositions which constitute the basis of his views are purely his own for, with the exception of Dr. Todd, and some of the experimentalists of the school of Vienna, they have certainly received no countenance from the lights of our profession. We do not find Alison, Christison, Watson, Copland, Jenner, Walshe, Barlow, etc., advocating such principles; nor have they thus far received the sanction of that test which all rules of practice must undergo—*the experience of the great mass of practitioners.* Closet-practitioners, and men striving for notoriety, may weave a thousand intangible but spacious theories, but it requires the crucible of clinical experience to separate the dross from the gold.

We might fairly leave this fifth proposition to its own inevitable fate, for having, I think, shown the fallacy of the hypotheses on which it rests, the conclusions must necessarily fall with the premises; but inasmuch as Dr. Bennett has reserved for this concluding part certain *statistical* evidence, embracing his own experience, it is proper to give it that attention which the importance of the subject so clearly merits.

Most observers perceive and acknowledge the difficulties and uncertain-

ties attending *statistics* connected with the practice of medicine; and we shall presently see to what extent these difficulties authorize us, in the main, to reject such evidence as faulty and unreliable. And however much we may admire the patience and industry of a Louis in attempting to create the *numerical system*, it must still be admitted that such facts are of necessity partial and inconclusive, and therefore unreliable data in the practice of medicine. In the language quoted by the distinguished Dr. Watson, it is better to *watch* than to *count*.

But as the opponents of depletory remedies in the treatment of inflammation depend largely on statistical facts to support their views, it becomes necessary to advert to this branch of the subject. The particular disease which has been seized upon is *pneumonia;* and it will be found, upon examination of the subject, that it has proved a very prolific, if not a very conclusive theme, and that the facts elicited are as contradictory as they are numerous. It is unnecessary to bring up in review all that has been published on this subject; but it will be sufficient to notice the most striking statements, and especially such as have been chiefly relied on by those opposed to bloodletting. In the *British and Foreign Medico-Chirurgical Review*, July, 1858, is a copious summary of this kind of evidence, from which I shall draw some materials bearing on this question; but it is the statements of contemporaneous observers, such as Bouillaud, Dietl, Wunderlich, Bennett, Bell, and Balfour, which are most valuable.

Bouillaud, of the French school, may fairly be considered, as *par excellence*, the leader of the bloodletting party of the present day; and his bleeding, *coup sur coup*, may be deemed an extreme test of the influence which *severe* depletion will exercise over pneumonia.

He thus states his method in pneumonia:—

"Bleed in the morning of the *first day* to sixteen, and in the evening to twelve or sixteen ounces. In the interval cup to the same amount, or apply thirty leeches. On the *second day* bleed again, and if pain still continues cup or leech. The disease, fortunately, for the most part yields on the *third day*. If otherwise, don't hesitate, but bleed again; but usually it is better to apply a large blister. As a rule you must not give up bleeding until fever, pain, and dyspnœa have almost ceased."[1]

Bouillaud states that he usually abstracted from four to five pounds of blood, the largest quantity being ten pounds, which was taken from a patient who recovered. He records the mortality to have been in 102 cases 1 in 8½.

This must be considered heroic bloodletting; and when we remember that it was indiscriminately employed, having but little reference to age, sex, stage of disease, or constitution of the patient, it may be considered as remarkable success, and exhibits a point of great significance in the treatment of the disease. If, indeed, bloodletting is so unconditionally evil, as

[1] Medico-Chirurgical Review, from Traité de Nosographie.

Dr. Bennett would have us believe, Bouillaud's patients should have all died; but, instead of this disastrous result, we find a mortality of only 1 in $8\frac{1}{2}$, which is fair success for hospital practice.

Let us now look at another extreme. Dr. Balfour observed the results of treatment in the Homœopathic Hospital of Vienna, under the care of Dr. Fleischmann, and states the mortality at 1 in $6\frac{2}{3}$. It will, of course, be conceded that homœopathic treatment is equivalent to non-interference, and we thus acquire some knowledge of the natural history of the disease. In this example, it appears, the mortality was large, which teaches two facts—that homœopathic treatment is unsuccessful, and that the disease in that instance tended largely to a fatal result.

Now, by contrasting Bouillaud's bloodletting with Fleischmann's negative treatment, it will be perceived that the results are favourable to the former; and if any induction is warrantable from such facts, it is that even excessive and indiscriminate bloodletting is better than unaided nature, and, therefore, depletion possesses positive curative powers.

The statistical evidence on which Dr. Bennett places most reliance (except his own) is that furnished by Dr. Dietl, of Vienna, which is classed under the head of "*treatment by diet.*" It appears that Dr. Dietl adopted three methods of experimental treatment: 1. By venesection. 2. By tartar emetic. 3. Dieting, with mild ptisans. The following is a general summary. Whole number treated, 380:—

1. By venesection 85, of whom 17 died = 1 in 5.
2. By tartar emetic 106, of whom 22 died = 1 in 5.22.
3. By diet 175, of whom 14 died = 1 in $13\frac{1}{2}$.

According to the face of this statement *nature* cures between two and three times as many cases of pneumonia as bleeding and antimony; *ergo*, bleeding and antimony should be abandoned, and the disease left to the recuperative powers of nature. There are, however, certain considerations which materially impair the force of Dietl's conclusions, and take from his figures their seeming authority. It will be remarked that Dr. Dietl treated his cases exclusively by a single remedy—bleeding or antimony—and these were evidently employed indiscriminately, and consequently must have been prejudicial in a certain proportion of the patients. Nor is it less important to remark, that no judicious practitioner would limit himself to a single agent, to be employed in all stages and conditions; on the contrary, he would resort to a combination of remedies to meet the indications of different cases and stages of disease. It is not surprising, therefore, that the indiscriminate use (or rather *abuse*) of a single *active* agent should give rise to a mortality equal to that reported by Dietl; nor is it at all strange that unaided nature should, in the aggregate, produce more favourable results than the palpable abuse of the most active agents we possess. Hence, Dr. Dietl's figures cease to be authoritative, and his conclusions must be regarded as fallacious.

In addition to these considerations, Dietl's conclusions are entirely neutralized by the reports of Wunderlich, of Liepsic. His cases may be thus stated:—

1. In 114 cases loss of blood occurred (by general and local bleeding, epistaxis, menstruation), of whom 9 died = 1 in 12.6.

2. 47 cases treated by venesection, 3 died = 1 in 15.6.

3. 76 cases without loss of blood, 13 died = 1 in 5.8.

It is also remarked by Professor Wunderlich, that bloodletting exercised a marked influence over the duration of the fever, or in promoting *defervescence.*

Without attaching any very great intrinsic importance to these statements, they are evidently entitled to as much credit as those of Dietl; and as they are precisely the reverse of the results obtained by the latter, they at least serve to place statistical inquiries in their true light, and to render us cautious in attaching too much confidence to *figures.*

But let us turn to the results of Dr. Bennett's practice. He informs us that he now abandons all effort to cut the disease short, but during excitement gives small doses of salines, *with the view of diminishing the viscosity of the blood;* and, as soon as the pulse becomes soft, orders beef-tea and wine. This course, the author states, he has carried on in the clinical wards of the Royal Infirmary during the last eight years, in which time he has treated 65 cases, of which 3 died, giving a mortality of 1 in 21⅔. The average age was 31; average duration of single uncomplicated cases 14½ days, and of the double uncomplicated, 21 days.

From these statistical statements, Dr. Bennett draws the following conclusions:—

1. That the result of a vigorous antiphlogistic treatment, as formerly practised, is a mortality of 1 in 3.

2. That the result of treatment by large doses of tartar emetic, according to Rasori and Dietl, is 1 in 5; but according to Laennec, 1 in 10.

3. That the result of moderate bleeding, as in the treatment of Grisolle, is a mortality of 1 in 6½.

4. The result of dietetic treatment, with occasional bleedings and emetics, by Skoda, is 1 in 7.

5. That Dietl's dietetic treatment gives a mortality of 1 in 13.

6. That Dr. Bennett's treatment gives a mortality of 1 in 21⅔.

It is evident from these statements that Dr. Bennett believes he has attained the highest degree of success in the treatment of pneumonia, and that Dietl stands next, the mortality being, respectively, 1 in 21⅔ and 1 in 13. We have the facts before us, however, to prove that other persons, and by depletory treatment, too, have secured still more favourable results, and, so far as statistics are reliable, establish the correctness of antiphlogistic treatment.

By referring to the *British and Foreign Medico-Chirurgical Review*, July, 1858, the following facts will be found:—

Reuf treated 94 cases of pneumonia by bleeding and antimony, of whom 5 died = 1 in $18\frac{4}{5}$.

Bang, of Copenhagen, treated 54 cases with antimony and bleeding, of whom 2 died = 1 in 27.

Trousseau treated 52 cases in Hôtel Dieu, with bleeding and antimony, 2 died, = 1 in 26.

Wossildo treated 76 cases, between the ages of seventeen and seventy (5 above sixty), by general and local bleeding and antimony, *none died!*

Burkart treated 60 cases by bloodletting, in 1854, when the type was inflammatory, with only one death, and that one was complicated with tubercles; hence he cured 59 uncomplicated cases without the loss of one.

We may now fairly ask, What is the basis of Dr. Bennett's conclusion, that his success has been greater than others? In vain may he appeal to former statistics of the Royal Infirmary, showing a mortality of 1 in 3, or to the results of treatment by Chomel, Louis, and Grisolle, where powerful remedies were almost indiscriminately employed; for, notwithstanding these statements, we find better results in the hands of other practitioners, and which equally deserve to be contrasted with the statistics of Dietl and Bennett. Thus, Dr. Bennett loses 1 in $21\frac{2}{5}$, while Trousseau exhibits a mortality of only 1 in 26; Bang, 1 in 27; Burkart, 1 in 59; Wossildo, 1 in 76! Surely, Dr. Bennett's success, compared with these triumphant results, dwindles to insignificance; and if we are to be guided by statistical evidence, then should we bleed and give antimony, to secure the most favourable rates of mortality.

The result of Dr. Bennett's treatment requires a special remark. In the first place, his cases were very few in number, being only 65 in eight years, which is evidence that pneumonia did not prevail as an epidemic, and consequently that he encountered only sporadic or mild cases. In addition to this, the patients were comparatively young, the average age being thirty-one; and 55 were uncomplicated, of which 40 were single. It is stated by Dr. Bennett, also, that six of the cases which recovered were bled and subjected to antiphlogistic treatment before entering the Infirmary—a fact of no small importance in estimating the general results. There is another significant fact which deserves to be noted in this connection. It is distinctly stated in the Review already referred to, that during the period covered by Dr. Bennett's cases, pneumonia was remarkably mild throughout Scotland, and that the mixed treatment (bleeding, antimony, and mercury) adopted by Dr. Bell, of Glasgow, was equally successful with the non-bleeding treatment by Bennett.

It will not, perhaps, be uninteresting to remark, that historical reminiscences often throw great light on the progress of medical doctrines; and

in connection with our present subject, a very brief retrospect will show that Dr. Bennett's antecedents have been very different from his present position. It is true, there is no offence against either good morals or sound philosophy in changing opinions as our views become enlarged and matured; but a change so complete and radical as exhibited in the present example, and based too, in both instances, on practical observations, is too remarkable to pass unnoticed. In fact, personal history becomes in some sense a part of science, especially when great innovations are based on individual experience. In his *Clinical Medicine*, published in February, 1858, Dr. Bennett declares that the course of treatment now recommended has been pursued by him for the past *eight years*. We find, however, in the *Edinburgh Monthly Journal*, August, 1851 (six and a half years before), the following language employed by Dr. Bennett in a clinical lecture:—

"I have on a former occasion pointed out the rule which, as it appears to me, should guide you with regard to bleeding in pneumonia. If you are called to a case at a very early period before exudation is poured out, and before dulness as its physical sign is characterized, but when, notwithstanding, there have been rigors, embarrassment of respiration, more or less pain in the side, and commencing crepitation, *then bleeding will often cut the disease short.*" (I have Italicized the most important part.)

How different Dr. Bennett in 1851, and Dr. Bennett in 1858! *Then* his clinical experience taught the important practical rule that bleeding, at the proper time, *would* cut short a pneumonia; *now* he emphatically declares that no such result can possibly ensue from depletory practice! In confirmation of his opinion held in 1851, he details one decided case in which the abstraction of ℥xv of blood was followed by marked relief, *even crepitation subsided*, and the patient rapidly recovered. The question, therefore, naturally arises, did Dr. Bennett observe, in 1851, that pneumonias were cut short by bleeding, or was he incompetent, as a diagnostician, to decide what actually occurred? Certainly we cannot doubt either his veracity, or his skill to trace the different stages of pneumonia, by the physical signs and general symptoms; and his observations at that period, unobscured as they were by a favourite theory, or deceptive statistics, will remain an incontrovertible disproof of his present speculative opinions. Nor is Dr. Bennett consistent with himself in his new position. He treats pneumonia, during the state of excitement, by salines, to diminish the viscosity of the blood; but, according to his theory, he should at the same time give brandy and beef-tea, in order to secure a large flow of blood. Truly, theory and practice do *not* always agree.

Some attempt has been made in our own country to develop the statistics of pneumonia. Dr. Austin Flint, Professor of Clinical Medicine in the New Orleans Medical College, has published a series of fifteen cases, occurring in the Charity Hospital, New Orleans, which were treated from the beginning with quinine, opium, stimulants and nutrients. It will be seen,

however, by reference to Dr. Flint's commentaries on the cases, that he does not claim them as a fair type of the disease in general, nor does he assume to exclude, in appropriate cases, depletory practice. On the contrary, the cases reported by Dr. Flint were, as he admits, evidently broken down in constitution, having long suffered with intermittent fever, and from the influences of exposure and a hot climate. We can readily understand, therefore, that these and similar examples, where miasmatic diseases prevail, may often require tonics and stimulants from the very onset, and entirely forbid all depletion.

Such facts, however, are far from establishing a general rule; for it is well known that pneumonia occurring under different climatic and endemic influences requires a very different course. This general fact is well illustrated in what occurred in the Louisville Marine Hospital, in 1855. The report for that year shows the number of cases to have been 37, of whom 18 died. I am not in possession of all the facts connected with these cases, but it has been stated that Dr. Flint, who had charge of the hospital during the winter, treated them mainly by the exhibition of opiates, and without bleeding, antimony, or mercury. It was, therefore, essentially an expectant and anodyne course; and the result contrasts strongly with the treatment in the New Orleans cases. I am not aware of any example on record in which the modifications of disease, demanding a corresponding difference of treatment, is more clearly exhibited than in these two classes of cases.

It becomes an important question for the practitioner to decide, how far confidence can be placed in medical statistics, especially such as bear on this subject, and to what extent such evidence can be made a guide in the treatment of disease? An examination of the statistics of pneumonia, which occupy so important a position in Dr. Bennett's theory, will reveal results so variable and contradictory as to deprive them of the slightest claim to authority. Thus, without depletion, Dr. Bennett's statistics show a mortality of 1 in $21\frac{1}{4}$; Dietl's, 1 in 13; the homœopathic 1 in 6; and the non-bleeding plan in Vienna, in 1856, 1 in 4. With antimony, bleeding, &c., Grisolle lost 1 in 8, Dr. Bell 1 in 17.7, Trousseau 1 in 26, Burkart 1 in 60, Wossildo none in 76. Pneumonia treated by inhalation of chloroform furnishes the following mortality: in the hands of Baumgärtner 1 in 10, Varrentrapp 1 in 23, Wucherer 1 in 90! In the Royal Infirmary, Edinburgh, former statistics show a mortality of 1 in 3; and this constitutes, mainly, the foundation for Dr. Bennett's denunciation of depletory treatment.

In addition to this, Kissel treated 112 cases, with a mortality of 5=1 in $22\frac{2}{5}$. When the urine was alkaline he gave iron; when it was acid he gave copper.

Here is exhibited a very wide range of figures. The non-bleediug plan varies from 1 in 4 to 1 in $21\frac{3}{4}$; the antiphlogistic from 1 in 3 to 1 in 90. Are not these results too variable to constitute any sound basis of practice?

If we take Dr. Bennett's statistics, we would certainly not deplete; if we take Wossildo's results as the guide, we will as certainly resort to blood-letting; but if we chance to adopt the tables of Wucherer, then we will administer chloroform! or iron and copper, if we depend on Kissel. Each partisan will find his theory fully sustained by these figures; but the judicious practitioner will perceive that some unseen agency has modified the results, and that the mere figures are but so many fallacies. It is evident, therefore, that the statistics of pneumonia, as a whole, are utterly worthless and unreliable as practical guides.

If we seek an explanation of these contradictory results in the treatment of pneumonia, it will be found in the numerous qualifying conditions connected with age, season, climate, epidemic and endemic influences, early treatment, stage, extent, and complications of the disease. And to these conditions we must add, in a general sense, the *individuality* of each case; indeed, so great are the differences in constitutions, that no two examples will exhibit the same characteristics throughout, nor will they admit of precisely the same method of treatment. And it is a due appreciation of these more minute shades of differences, as well as the broad distinctions observed in the varying *forms* of the disease, that constitutes the truly skilful physician, and which enables him to meet the emergencies of each case, instead of relying on conclusions drawn from *groups* of cases.

Viewing nationalities in a somewhat prejudiced light, a critical writer intimates that the English think more of some other case than the one under treatment, while the French think more of the disease than of the patient; hence the former individualize the disease, the latter generalize the patient; but the true course is that indicated by Hufeland, to *generalize the disease and individualize the patient.* It is quite immaterial to our present purposes, whether these distinctions exist among French, Germans, and English, or not; but we cannot fail to observe their strong development in individual writers. Statisticians rob each case of its individuality, and cast it upon the sea of uncertainties pertaining to others of a different character. Thus one series will all be bled, another will receive tartar emetic, and a third left to the chances of nature. In the first class, some are bled who should have been stimulated; in the second, tartar emetic is administered when bleeding would have been preferable; and in the third class, some are permitted to die from mere over-action. In this blundering, if not criminal procedure, individuality is ignored, and the practitioner prescribes for a mere *name*, leaving the patient to the mercies of chance or fate.

It is evident, therefore, that a rational treatment must secure to each case its own individuality; and as the shades of differences, and the corresponding modifications of treatment cannot be expressed in *groups*, statistics, in this sense, become simply an impossibility. For example, bleeding, antimony, mercury, and blisters, may be demanded in one case;

quinine, opium, and wine in the next; a third may require but little interference, except a well-regulated diet with moderate stimulants; and so on, *ad infinitum*. The treatment of pneumonia demands not a single but many agents; and he who would attempt to develop results by statistics, will be required to make each group a *unit*. It is the proper *combination* of remedies, and not a single agent or mode of practice, which is capable of securing the best results in the treatment of disease.

The preceding considerations render it abundantly evident, that mere statistical tables have not furnished the class of facts on which practitioners can rely in the selection of remedies; and, indeed, the only important revelation which has been made is, that, under certain conditions pneumonia manifests a stronger tendency to spontaneous recovery than could otherwise have been known. Dietl, for example, under a system of mere diet, reports the mortality of only 1 in 13, which is more favourable than resulted from the systematic course pursued in Paris. But the presumption is very strong that Dietl's cases were of that grade which tend to spontaneous recovery; at least we have the important contrary fact, that in 1856, the non-bleeding treatment of pneumonia in Vienna was very unsuccessful, the mortality being 1 in 4! Doubtless, however, uncomplicated pneumonia, when single and occurring in good constitutions, would generally recover without the aid of medicines; *and the same is true of a large proportion of other diseases*. Many cases of typhoid fever, measles, scarlatina, smallpox, and so on, would recover without the intervention of art, or the aid of a physician. But these facts establish nothing in the premises; nor can any evidence be adduced to prove that suffering may not be mitigated, and life often saved by the timely and judicious application of remedial agents. Some examples of disease may safely be intrusted to the recuperative powers of nature; but even in this sense there are few cases which might not be benefited by proper medication. Some cases require strong remedies, others weak ones; and it is the function of the enlightened physician to determine when his strong and when his weaker agents are to be brought into requisition; when he is to trust mainly to the *vis medicatrix naturæ;* when to sustain the faltering powers of life; or when to subdue vehement action, which, by its own violence, threatens destruction to the animal economy.

It has been observed, however, that the treatment of inflammatory diseases has materially changed within recent periods, depletion having fallen into comparative disuse, and that this important modification is due to a better comprehension of morbid action. This observation, although only partially true, might be admitted without solving the question at issue. Medical science, and, based on this, medical *art*, is progressive; we, in modern times, know more of pathological changes and therapeutical actions than did our predecessors; and hence we measure the influence of the one by the character and degree of the other with more accuracy than formerly.

The natural history of disease has been more carefully studied; and its tendency to spontaneous cure, or a fatal termination, materially modify therapeutical applications. Hence, a juster comprehension of pathological changes, and a broader and more enlightened experience, have taught us that essential fever is not to be cured by depletion; that remittent fever is, as a rule, more efficiently treated by quinine than mercury and bleeding; and so, too, have we learned that excessive depletion in pneumonia may often be dispensed with, and a more conservative course advantageously adopted.

But the question recurs, whether the diminished employment of blood-letting has resulted exclusively from the advanced state of pathology, or whether it is not due to a change in the *type* of disease, whereby depletion is less frequently demanded. Drs. Alison, Christison, Watson, and others, declare that the type of inflammatory affections has changed, having become less *sthenic*, and, consequently, the lowered grade of action no longer requires copious depletion; while on the contrary, all this is denied by Dr. Bennett, who asserts that no change of type has occurred, but that the modification of practice is due to a more enlightened pathology; and on that advanced pathology he predicates his peculiar views.

These are questions not easily settled by the mere pathologist or the logician. Dr. Bennett may demonstrate that cell-action, and the laws governing the process of exudation, are the same now that they were in the days of Cullen and Gregory, or even of Hippocrates and Æsculapius; but the observation is radically defective, because it ceases precisely where it should have begun. The demonstration should ascend from the local affection to the constitutional reaction; and thus by measuring the degree of excitement, reveal the diathesis of the disease. In fact, the evidence that the type of disease fluctuates in intensity, must be derived from personal observation and experience, in relation to the conditions of the general system, rather than the revelations of the scalpel or the microscope, concerning the minute changes in inflammation. And it is no more possible to establish such a fact by *statistical* evidence, than it would be in the same manner to prove the exact agency of psychological influences in the production or aggravation of disease. But there is an experience and observation altogether above mere arithmetical calculations; and it is from this broad and reliable evidence that we learn the fluctuations of disease in different seasons, years, and periods of time.

The variation of inflammatory affections may be clearly observed, on a limited scale, in what occurs during the different seasons of a single year. Thus it is well known that inflammatory diseases bear and require more antiphlogistic treatment during winter than summer. But still more distinctly are these variations observable in different years; indeed, every practitioner must have remarked that the same classes of disease manifest a much higher grade of action, and require more depletion during some years

than others. And, if this is true of seasons and years, there is no obvious reason why the same influences may not extend through longer periods or cycles of time. In our own country we have numerous illustrations bearing on this question of the change of the type and character of disease. Thus it is well known that, since the prevalence of Asiatic cholera in 1832, there has been manifested a greater degree of irritability of the alimentary canal, and consequently diminished tolerance of cathartic medicine. Purgatives have fallen into disuse, since the days of Hamilton, even to a greater extent than has bloodletting since the days of Cullen and Gregory. And it may be safely affirmed that the change of practice in this respect cannot be ascribed to an improved pathology, but to a broad and enlightened experience growing out of an obvious change in the *type* of disease.

In the western and southern portions of the United States another and even more striking change has occurred. The endemic fevers of this vast region were originally of the periodical type; but as early as 1842 we were invaded by well-defined continued (typhoid) fever, which in many localities superseded the periodical fevers. The continued type predominated in many localities for a period of ten years, since which time it has gradually diminished, while periodical fever again becomes more common. I do not assert that these changes were radical and complete in every district, but the predominance of the two types occurred, as I have stated, in many regions of country, and the typhoid element seems to have permanently impressed most of the diseases incident to the climate. And this important modification of disease demanded at once a radical change of treatment. The preparations of bark and mercury, together with bloodletting, were no longer efficient; but instead of these, the employment of stimulants and nutrients became the leading agents. Quinine, so efficacious in periodical fevers, was not only inefficient in the new form of disease, but was often found positively pernicious; mercury was seldom required, and frequently wholly inadmissible. This great change of treatment was not due to an improved pathology, but it arose from the introduction of a new form of disease, and experience soon indicated the necessary changes of treatment.

But still another modification in the type of disease has occurred here and elsewhere. Practitioners have observed, for some years past, a *nervous* type, with often a decided tendency to prostration, so much so, indeed, that depletion must be resorted to cautiously, or entirely interdicted.

Now it is evident, from the concurrent statements of writers in Europe and America, that these changes are general and common to both countries; and that a corresponding modification of treatment has occurred, co-extensive with the changes in the type of disease. Typhoid fever has of late years spread over England, and has been fully recognized by the practitioners generally. In 1845, the writer of these remarks observed cases of typhoid fever in the London Fever Hospital; but they were limited in number, and, perhaps, not well defined, for Dr. Tweedie remarked that

the disease had not been recognized, and that they made no distinction between the typhus and typhoid forms. Soon, however, typhoid fever multiplied, and its existence was fully recognized by Dr. Jenner and other observers.

I may also mention here, as evidence of the change which the *type* of fever undergoes, that Dr. Tweedie's report for the year 1845, shows the low form of disease by the large quantity of stimulants demanded. He states that, in the epidemic of 1843, when 1,100 patients were admitted, the quantity of wine administered was about 1,800 ounces, and 60 of brandy; while the next year, although not half the number were admitted, they consumed 14,000 ounces of wine, and 760 of brandy, besides gin and porter! No fact could be more striking and conclusive than this.

It ceases, therefore, to be a matter of surprise or doubt that these zymotic causes, with others probably unknown, should modify the *type* of disease and require treatment greatly changed in character. The choleraic and typhous poisons, to say nothing of the causes which have so extensively modified the nervous system, must be regarded as fully competent to effect these important changes.

It is contended, however, by Dr. Bennett, that *inflammation* is always essentially the same, and hence there has been no change in the *type* of disease. It is very true, indeed, that the elementary actions characteristic of inflammation must necessarily remain unchanged; that is, the adhesion of the corpuscles, distension of vessels, stagnation of blood, and, finally, the exudation of lymph, are the same in the days of Bennett that they were in those of Cullen. But this does not embrace the main question, for it is not the local changes occurring in an inflamed tissue which are supposed to have undergone changes, but it is the condition of the *general system*—of innervation, circulation, and all the vital functions. These become, from general causes, depressed; and although there may be no change in the microscopic appearances of inflammation, nevertheless the *reaction* is less intense, the tendency is to depression, and depletion is less demanded. Hence the *type* of disease may change, while the minute process of inflammation remains unaltered.

It is not contended, however, that this change of type will be observed in every example of disease; on the contrary, we still witness the old division of sthenic and asthenic inflammations. The former, however, have diminished, until, finally, the latter predominate; or what would, perhaps, be more correct, there is a general lowering of the grade of action, which requires less depletion than did the same classes of disease in former years.

At the same time, I am strongly inclined to believe that the great outcry against bleeding has driven us to the opposite extreme, and we now deplete less than the interests of our patients frequently require. With the prevailing aversion to bleeding, cases are liable to be overlooked, and deple-

tion neglected from sheer habit. Dr. Christison clearly proves, within his own personal experience, that the synocha of Cullen has several times recurred, and each time demanding depletion. But he who would regard that form of fever as a *myth*, would not recognize its new introduction, and therefore would fail to meet its exigencies.

The question of bloodletting in inflammation has been discussed almost exclusively in relation to pneumonia; but, notwithstanding the evident good effects of depletion in certain forms of that disease, it is not the affection which most clearly illustrates the powers of depressing treatment. The reason of this is, that pneumonia is so exceedingly variable in its tendencies as to defy the most careful observer in his attempts to tabulate results. But for the purpose of clearer illustrations, let us apply the same principles to other forms of inflammation, such, for example, as encephalitis, hepatitis, peritonitis, and gastritis. Would it be contended that, in a case of inflammation of the brain, characterized by a full and strong pulse, throbbing carotids, active delirium, etc., *stimulants* and *nutrients* should be employed, for the purpose of promoting cell-action? It would, probably, be difficult to find a practitioner, since the days of Asclepiades, not excepting Dr. Bennett himself, who would not bleed in active phrenitis, cover the abdomen with leeches in abdominal inflammation, or apply cups in hepatitis. And if this be true, the same *principles* should regulate the treatment of pneumonia, varied according to the tendencies of individual cases. In an example of active pneumonia, occurring in a robust constitution, with full reaction, hot skin, and oppressed breathing, no physician would dare withhold depletion and substitute stimulation. The instincts of our nature, to say nothing of science, would forbid it, and the opinions of enlightened physicians would declare it *malpractice*.

The treatment of pneumonia, I need scarcely add, must vary with its forms and modifications. Let us assume the existence of the following varieties:—

1. Sthenic pneumonia.
2. Asthenic pneumonia.
3. Latent pneumonia.
4. Specific pneumonia (typhoid, miasmatic, etc.).
5. Diathetic pneumonia (rheumatic, scrofulous, etc.).

In these five species will be found therapeutical indications widely different; and the discriminating practitioner will perceive the necessity of bleeding in one, of giving quinine and opium in another, while still another class will demand specific treatment. It is freely admitted that in the milder forms of pneumonia but little treatment is demanded, and certainly bloodletting may often be omitted; but in the graver varieties the agents must be more active, or the patients will be destroyed by the inherent force of the disease. In the milder forms of all diseases, the *vis naturæ* may be

sufficient to overcome morbid action; in others, again, the same *vis naturæ* must be protected from the destructive tendencies of *over-action;* while in another class, characterized by debility, the powers of nature must be sustained by stimulating agents. These are the guides which physicians, unbiased by speculative doctrines, always recognize, and which constitute the basis of enlightened practice.

The denunciation of bloodletting is nothing new. From the days of Chrysippus and Erasistratus to those of Bennett and Dietl, parties have often risen and flourished on a species of monomania on the subject of bleeding. In Rome, in the days of Galen, the opposition to depletion was fierce and bitter, and the *blood-funkers*,[1] as they were humorously called, so far predominated as to prevent depletion in the most urgent cases. And, finally, the predominance of Arabian over Greek medicine caused bloodletting, in any efficient form, to be abandoned. But, in the beginning of the sixteenth century, Pierre Brissot, a physician of Paris, finding most of his pleuritic patients die, ventured to revive the Greek practice of bleeding. This created, says Renouard, a great uproar in the medical world, and violent controversies ensued. And more recently, the Brunonian doctrine, although it does entirely exclude bleeding, is based measurably on the employment of stimulants to remove *debility.* In our own country the outcry of the blood-funkers has been as excruciating as the *antimonial martyrology* of Guy Patin. Originating in the profoundest depths of ignorance and presumption, it has gradually risen to a more imposing but not less false position; and while struggling to sustain a system based on the relics of the dark ages of medicine, such as Galen and Brissot had to combat, the Edinburgh defection comes opportunely to their support. Dr. Bennett may have the satisfaction of knowing that his doctrines, in the United States, are eagerly embraced by the empirics, while the regular profession repudiate them as false and mischievous. The American *blood-funkers* find great consolation in the doctrines of the Edinburgh and German medical *Illuminati.*

But let us hope the end is not yet. Theory and practice do not always accord. The celebrated blood-funker has been attacked in a tender way. Inflammation came stealthily upon him, when lo! Dr. Bennett's case *demands bloodletting!* His colleague, Professor Miller, informs us that his sthenic constitution nobly sustained depletion. Thus, the hand of Providence becomes a more potent teacher than statistical tables or microscopic revelations.

[1] Chambers, Med.-Chir. Rev., Oct. 1858.

ART. II.—*Some Remarks on the Adynamic Type of Remittent Fever and its Treatment with Nitric Acid.* By BEDFORD BROWN, M. D., Yanceyville, Caswell County, N. C.

ATTACKS of remittent fever, apparently mild in the beginning, may subsequently assume the adynamic or typhoid type at any period of their progress. Yet this type, in its most dreaded and malignant form, generally characterizes the attack of fever, from the very commencement. When developed fully and in all of its intensity, it is truly a disease of fearful import. Indeed, in point of malignancy, it is but little inferior to the worst forms of typhus—a disease to which it presents features of striking resemblance.

Features peculiar to the Adynamic Type of Remittent Fever.—The various symptoms and indications of ordinary remitting fever are so materially modified by the presence of adynamia, as to give them a character utterly different from the original. The tendency of this modification is to create a very decided depression of the vital functions.

The organic properties of the blood, as well as the vital condition of the general organization, are at a reduced standard. Consequently, as might be expected, vital action is invariably impaired in a corresponding ratio.

In the most malignant grades of this type, observed by myself, those vital attributes, termed "sensibility and irritability," are diminished to an extent that would attract the attention of the most unobserving. The slow and feeble pulse tells plainly how the imperfectly nourished cardiac tissue fails to respond vigorously to the stimulus of impure blood. Again, the fever is of an inferior and imperfectly developed grade, as is manifested by the short and irregular exacerbations, the long and prostrating remissions, in which the animal temperature is reduced far below the healthy standard. Nor are these remissions attended with general perspiration. Neither do they afford that signal relief to the patient which we witness in milder cases. The appearance of the tongue is peculiar. In other instances we view redness of this organ as of evil omen; but in this, the tongue that is *clean*, pale, dry, and harsh to the touch, is infinitely more to be dreaded. The state of the mental faculties in these cases is greatly modified and changed. The powers of memory are utterly confused, and frequently lost—particularly the recollection of time and place. The control of the mind over the voluntary operations is irregular and defective. Within a brief period after the attack, the patient is often unable to protrude the tongue. Nor is he often able to assume the upright posture, from danger of fatal syncope.

In this class of cases delirium is by no means a marked symptom, and when present approaches nearer in character to gentle monomania. To the contrary the mind seems generally incapable of acting at all. In all

somnolence and stupor are present. The patient makes no complaints; he neither inquires nor wishes for anything. I do not remember that in any of these cases abdominal symptoms were very prominent. Constipation usually attended them in the beginning, and occasionally throughout the progress of the disease. Diarrhœa, though generally present in the latter stages, was not troublesome at any time. Tympanitis was not common or excessive. There was usually some abdominal tenderness on pressure. Emaciation and reduction of the tissues were strikingly rapid in all cases. The skin and muscles presented a withered appearance, as if the fluids had been drained from them. Subsultus, muscæ volitantes, and vigilance were frequently present. In many the vision was very imperfect. Sensation to morbid impression was invariably blunted. Pain after the early stage was rarely complained of. Unless relieved these cases usually terminated rapidly. In another grade of adynamic remittent fever, and one less dangerous than the preceding, the development of the peculiar symptoms was both more gradual and insidious. The progressive tendency towards this state could be observed increasing from day to day in cases that had previously pursued a mild course;—a pulse becoming daily more frequent and feeble;—a tongue exceedingly dry, and presenting a scarlet hue;—delirium frequently wild and furious; the patient often attempting to seize imaginary objects or make his escape from the bed. In this class of cases tympanitis, diarrhœa, and abdominal pain and tenderness were rarely absent. These attacks pursued a course in general features scarcely distinguishable from typhoid or enteric fever, being always protracted and tedious.

My own experience being limited in the morbid anatomy of this disease, I am compelled to draw on reports of the investigations of others in analogous cases for information in this particular.

Morbid Character of the Blood in Adynamic Remittent Fever.—The reports of all those who have investigated the subject go to confirm the opinion that there is an absolute deficiency of fibrinous or plastic material in the blood of adynamic patients. The accounts to which I have had access report the quantity of fibrin as far below the normal standard. They also report a remarkable defect in the plasticity or coagulability of the plasma; the blood, with a few exceptions, being found in a fluid and uncoagulated condition after death. In these exceptions there were a few small coagula detected, which were of a soft and semi-fluid consistence, without possessing any of the properties of firmness or cohesiveness belonging to healthy fibrinous concretion. These points are of great importance, both for the establishment of a correct knowledge of the pathology of fever, and of sound principles for our guidance in its treatment. Indeed, when we consider that it is from the blood that the human fabric is built up—formed and sustained in all of its mysterious intricacies, we can well comprehend and appreciate the close and inseparable relationship existing between them. The merest variation or change in the one, must materially influence the

condition of the other. Whilst all the constituents of the blood are important to organic and animal existence, a certain portion are essentially necessary. In the blood plasma the albuminous properties usually predominate in quantity; but those of a fibrinous character probably play the most important part in the construction of the organism. From fibrin is constructed the framework of the economy—all other properties entering the organization answering a subservient purpose. Could, then, the amount of mischief be calculated that would result from a rapid withdrawal of a substance that enters so largely into the composition of the body?

While we have no positive knowledge to guide us, we have a right to presume that the albuminous properties suffer equally with the fibrinous in adynamic conditions. No cause can be assigned for these changes in the plasma, other than the deleterious action of the poison of miasma or contagion. The blood assumes a condition that would almost rank with inorganic substances, and is, therefore, entirely unsuited to supply material to sustain the organization.

Morbid Anatomy of Adynamic Remittent Fever.—While after certain acute attacks of remittent fever positive evidences of congestion or inflammatory action are often detected in the principal organs, in the malignant or adynamic forms of the disease the most uniform and constant organic change discovered is softening of the tissues. Reports of researches by all authors made on this subject go to establish and confirm this fact. Another important point in relation to the subject of softening of the structures in these cases, is the entire absence of any evidences of inflammatory lesions found in connection with it. It appears, under these circumstances, to be a lesion resulting purely from mal-nutrition of the organism. And what is remarkable, according to reports, these organs are more liable to this degeneration, into the composition of which fibrin and albumen usually enter most freely. After fatal cases of malignant remittent fever the muscular tissue of the heart is almost uniformly found in a softened and flabby condition.

In a large majority of the post-mortem examinations made by Dr. Stewardson, of Philadelphia, this condition of the cardiac substance was detected.

In most of the fatal cases examined and reported by Drs. Anderson and Frick, of Baltimore, the tissue of the heart was *softened, pale, and flabby.*

Maillot, in his reports of post-mortem examinations of the victims of this form of disease, refers in positive and emphatic terms to the "*remarkably pale and softened* state of the heart." The brain has also been frequently found in a softened condition. In reference again to Dr. Stewardson's reports, the condition of the liver was observed softened and changed in colour, so constantly as to induce him to consider it the "peculiar lesion" of the disease. According, also, to the observation of Dr. Swett, of

New York, the consistence of this organ was found softer than natural in nearly every case.

Again, the reports of Drs. Anderson and Frick on this point are in positive confirmation of the same fact. During the progress of this disease the structures composing the alimentary canal are equally subject to the same lesion. The spleen as often partakes of a similar condition. One among the commonest occurrences in the pathology of this form of fever, is wasting and softening of the entire system of voluntary muscles. A pathological change which is found so often pervading the various tissues, certainly cannot depend on causes of a local character. Its very nature seems to be antagonistic to inflammatory action. It is a condition by no means confined to this particular malady; but appears to be equally common in all kinds of malignant fever. Yet so far as my knowledge extends, under whatever circumstances detected, it is always found in intimate association with a circulating blood, which has lost capacity for organization. It appears from well-attested facts to be a condition by no means incompatible with complete recovery, and a return of the tissues affected with it to their original integrity. When we take into consideration the peculiar state of the blood, with which this particular lesion is so uniformly found in connection, the conclusion irresistibly forces itself upon our minds that they stand in the relationship of cause and effect. In fact there is every reason for the belief that this condition is one of true molecular disorganization, resulting from a deficient supply of plastic material for the proper renovation of tissue.

Relationship of this Lesion to particular Symptoms.—When the structures of the various vital organs have undergone that molecular change denominated softening, either partial or entire, the functions peculiar to such organs are modified in a degree proportionate to the extent of organic alteration. That this should be the case is not at all surprising. It is more than probable that there can be no functional deviation from health under any circumstance, without the previous occurrence of some organic change to account for it. We might, then, very rationally conclude that when, in an ordinary case of remittent fever, the organization undergoes this alteration the features peculiar to it would assume an entirely different aspect.

That the action of a softened and flabby heart should produce a pulse, feeble, irregular, and slow or frequent, according to the degree of lesion; that the respiration should become painfully difficult and embarrassed on the least muscular exertion; that the patient should be threatened with syncope when assuming the upright posture from an inability of the heart to sustain a perpendicular column of blood, or that there should be impairment and even complete obliteration of the mental faculties, as is evinced by delirium, somnolence, and coma, when a proper renovation of the nervous tissue has partially or entirely ceased and molecular disorganization begins,

are results which might reasonably be anticipated, and do in reality occur. Again, neither is a softened and attenuated muscle capable of healthy action, a stomach of digesting, a liver of secreting healthy bile, or a kidney urine. Neither has the blood materials fit for the formation of normal secretions.

That blood should frequently exude from vessels, the consistency of whose coats have been diminished, and create alarming hemorrhages, is an occurrence reasonably to be expected, and in fact one that does often occur.

One of the most characteristic and uniform results of this organic change is constant and excessive prostration of the mental, organic, and animal functions. The mind is frequently incapable of originating a thought or recollecting a recent event. Nor can it will or direct the movement of a limb. The vital operations of respiration, circulation, secretion, and assimilation are sluggish and imperfectly performed.

Prognosis.—Cases of remittent fever characterized by adynamia from the very commencement are always of extreme danger. When this type supervenes at a later period of the disease, the condition, though less dangerous, is still of grave import. In the most malignant cases that have come under my observation, delirium was not always present, and when so, was not constant or regular. The character of the pulse, as a prognostic criterion, could not be relied on alone. While in many of the most alarming cases the pulse was very frequent, in others, probably of a more malignant grade, it was *very feeble and slow.* Many cases recovered in which the pulse reached at some period of the attack one hundred and forty. The volume of the pulse afforded more valuable information generally than the frequency of it. In some cases, just at the approach of convalescence, the pulse became irregular and intermitting. Frequency of pulse, when developed very early, was a serious indication. A very variable pulse was an unfavourable sign. When the pulse diminished in frequency, and expanded in volume in a decided manner during the remission, the disease pursued a more moderate course. A tongue that was constantly *pale, dry, and clean* appeared in connection with some of the most malignant cases I have ever witnessed. A red hue combined with dryness was a less unfavourable indication, and when moisture was present the case usually terminated in health. Among the earliest indications of this termination was a slight return of moisture and softness to the tongue. Constipation was frequent in the worst cases, while diarrhœa, to some extent, was present in the latter stages of the milder cases. I do not remember that this symptom was a troublesome one in but a few cases. In the milder grade of adynamia, tympanitis was more common than in those of a more rapid and fatal character. Imperfect and irregular febrile reaction was a condition always to be dreaded. Also material and prolonged reduction of temperature. A uniform coating over the tongue was invariably associated with the mild cases. Whereas in all those of a malignant type it was clean or but slightly

coated. When the case assumed a milder form, a white coat spread gradually over the tongue.

Treatment.—Residing in a country in which the prevalence of remittent fever is an annual occurrence, ample opportunity has been afforded me of testing the efficacy of the various remedies and modes of treatment adopted and recommended by authors for the cure of that disease. While a large majority of patients, suffering from attacks of the simple and inflammatory forms of the disease, will recover under the use of such means as local and general bleeding, moderate purgation, sulph. quinia, mercury, oil of turpentine, &c., these remedies have proved utterly inefficient in the adynamia or malignant types. Indeed, it seems to me that under this system of treatment, the patient's ultimate recovery is more a question of constitutional vigour and power of resistance to disease than of remedial efficacy.

Believing that morbid anatomy and a careful observance of the vital operations, as modified in their action by certain molecular changes of a morbid character, afford us satisfactory evidence that the blood in the adynamic types has undergone a degenerative process; that its organizable properties are in a depreciated condition and are unfit for assimilation, there results consequently a decided tendency to that molecular disorganization of the solids, termed softening. To prevent the formation of this type in remittent fever, and to relieve it when established, has ever been a desideratum with the profession. Under these circumstances it is reasonable that we should seek for a plan of treatment to suit our peculiar views, and one of a more efficient character.

The tonic properties of nitric acid have long been known to the profession. Recent experiments with it by a western practitioner, in the treatment of ague and fever, have fully established the virtues of the same remedy as an antiperiodic. The tonic properties of the acid are not due simply to any influence which it may exert on the digestive organs. But that it aids in the formation of the blood, and renders it more plastic and organizable, cannot be doubted. Dr. Williams, of London, remarks on it in other diseases thus: "A chief part of the benefits arising from the use of stimulants and tonics may be fairly traced to their promoting the healthy formation of blood and an increased vigour of the circulation; but there are some agents which seem to augment the plasticity of the blood, and have no very remarkable tonic powers; I allude to nitric acid, nitro-muriatic acid, &c."

With these considerations in view, I resorted to the use of this remedy in the treatment of remittent fevers of an adynamic type. The powers of the remedy have been satisfactorily tested in at least forty cases by myself and partner, Dr. Roan. The acid was administered in simple cases, in connection with sulphate of quinia and other minor remedies. To the quinia it has certainly added efficacy. A large proportion of the cases were of a decidedly malignant type. In a sufficient number of these to determine its value the acid was administered alone, with the exception of a few remedies

of minor importance. I do not remember a single fatal case, after its free and constant use. I am also impressed with the belief that it possesses much power to prevent the development of adynamia when given early. Patients, after the use of it, recovered more rapidly and completely than under the usual treatment. Under the influence of the acid it was pleasing to witness the change of form to a more simple character, by a successive clearing up and subsidence of the alarming symptoms. For the abdominal complications of certain cases I have found the internal use of nitrate of silver valuable. Symptoms of these complications I have seen again and again disappear under its influence.

The nitric acid was usually administered in doses of three drops every six hours, and gradually increased according to the intensity of the case—occasionally as high as ten drops. It was given sufficiently diluted in sweetened water to render it grateful. That the mode of treatment adopted may be better understood, I have thought proper to give a general description of a few of the worst cases treated.

CASE I. This was the first instance of the kind in which I tested the remedial powers of nitric acid. The case had progressed to a considerable extent when first visited. In the beginning it was one of a simple form of remitting fever, but from neglect had assumed an alarming degree of malignancy. The subject of it was a white adult, aged 25. His pulse was excessively feeble, and ranging between eighty and ninety. The tongue very *dry, brown,* and *fissured.* His mind was forgetful, and frequently wandering. He was usually somnolent and indifferent to objects around him, and to the issue of his case. Constant subsultus and vigilance. A troublesome diarrhœa was present, with abdominal pain and tenderness. These symptoms were further aggravated by tympanitis. The skin was constantly hot and devoid of perspiration. Emaciation had advanced to a considerable extent, and vital prostration to a manifest degree. In this case nitric acid was administered in doses of three drops every six hours, and before recovery was gradually increased to ten.

The chief remedy addressed to the abdominal complications was nitrate of silver with anodynes. There is a subacute inflammatory condition of the intestinal tube, which is frequently developed during the course of some of these cases, over the progress of which the nit. argent. exerts a control equal, if not superior, to what it does in the ulceration of Peyer's glands in typhoid fever. I am constrained to believe that this efficacy is increased by the coadministration of the acid. It is possible that the supposed decomposition of the salt in the stomach is in this way prevented. In connection with other remedial measures, the patient was allowed the use of alcoholic stimulants and concentrated nutriment, as freely as his case required.

Under the combined influence of these means, the malignant features began to yield in a few days. The case assumed a mild type, and ultimately recovered.

CASE II. A coloured boy, aged 12. This was a much more serious case than the first. When first visited, on the second day of his illness, he had a pulse of one hundred and forty. Tongue very red, and disposed to be dry. Much abdominal tenderness on pressure, and constipation. Febrile

reaction decided. Stupefaction of the mind to so great a degree as to prevent his comprehension of the questions addressed to him. There were morning remissions without perspiration. He was treated the first three or four days with gentle purgatives, cold applications to the head, sulphate of quinia during the remissions, and to control the excessive excitement of the circulation, and obviate the tendency to cerebral complication, the veratrum viride was administered. These objects were partially accomplished so long as the remedies were continued; but the equal tendency to adynamia or malignancy was not prevented. The case began to assume this type about the fifth day. His tongue became perfectly dry, brown, and crusty. It was so hard and contracted as to resemble flesh that had been subjected to the action of heat. The pulse diminished in force, while it increased in frequency. Fever assumed the continued form—incessant delirium, with an uncontrollable desire to escape from the house, diarrhœa, tympanitis, and tenderness over the abdomen. I now determined to adopt a different mode of treatment, with the sanction of my partner, Dr. Roan.

Nitric acid was given in three drop doses every six hours. Nitrate of silver one-quarter of a grain three times daily, with a liberal amount of nutriment and alcoholic stimulants. The improvement in all the malignant symptoms of this case, at the end of ten days, was remarkable. The type had undergone an entire change to that of the mildest form of remittent. One of the earliest and most noted perceptible alterations was in the condition of the tongue. As that organ became softer, paler, and more moist, all the alarming indications vanished, as if by magic. The convalescence of this patient was steady and permanent.

CASE III. This case, in general character, was almost identical with the second; the patient a boy, eight years old. Malignant symptoms supervened at a very early period of the attack. Of all these, I do not remember that there was a single one of favourable import. His delirium resembled that of a fierce lunatic; while, with hot skin and dusky complexion, the pulse was very feeble and rapid. Sordes accumulated about the mouth. His tongue became so dry and indurated as to prevent protrusion. Extreme tympanitis set in, with an annoying diarrhœa, which further increased the physical prostration; involuntary discharge of urine and feces. Under the influence of the acid and nitrate of silver, with stimulants and nourishment, this case steadily improved, assuming a very mild form, with very decided remissions and perspirations. The sulphate of quinia was then added to the other remedies with marked benefit. This little patient made a recovery that was never anticipated.

The two last cases presented one peculiarity conflicting with our preconceived notions of the relationship of lesions and symptoms. The extreme disturbance of the brain in both might have impressed us with the conviction of a dangerous amount of inflammatory action. But the treatment and its results proved to the contrary. To the efficacy of the treatment adopted in many of these cases, Dr. Roan, a gentleman of large experience and excellent judgment, can bear testimony.

CASE IV. A female servant, aged 26. This case was remarkable for the malignancy of type, and also for its association with an attack of constitutional syphilis, with which the patient had suffered for some time previous

to the appearance of fever. Doubtless much of this malignancy was due to the combined influence of both affections. While going about, and for some days before confinement to bed, the patient's mind was observed to be impaired and occasionally she was delirious. This delirium continued for six weeks. When she came under our observation at the commencement of the second week of her sickness, the following symptoms were present: The condition of her mental faculties amounted almost to dementia. The pulse was excessively feeble, and not exceeding ninety. Fever of a very low grade, with nocturnal exacerbations, followed by protracted remissions, during which the temperature sank to a very low degree. Her tongue was never coated nor dry at any period; but was as tremulous when an attempt was made to protrude it as that of a person suffering with mania-à-potu. This tremulousness extended to the entire muscular system. Diarrhœa was both troublesome and exhausting. Towards the latter stages emaciation became extreme. The skin became covered with large sloughs and ulcers, probably to the number of thirty. These sloughs formed more particularly about the large points. Some of them measured three inches across. When about to form, a small vesicle appeared under the cuticle, a circular slough of the skin then rapidly formed, and, after separating, left a deep and clean excavation, often extending to the muscles beneath. While these sloughs formed more readily on points exposed to pressure, they appeared indiscriminately over the surface about the large joints, back, &c. They neither granulated nor suppurated until the beginning of convalescence. In this case muscular prostration was extreme. When the patient was propped up in bed occasionally for a few moments, the heart would almost cease to act; the slightest muscular exertion greatly embarrassed her respiration.

Nitric acid, nitrate of silver, occasional opiates, and diffusible stimulants, with a sufficiency of nourishment, were administered, and continued during her entire illness—which was of more than two months' duration—with the exception of about a week, when she took iodide of potassium. The sloughs and ulcers assumed so aggravated a character about that period as to demand some antisyphilitic remedy. Under the iodide of potassium the ulcers improved perceptibly. A further increase of the adynamic symptoms compelled us to suspend this remedy, and again resort to the acid a second time. The patient is now to all appearance convalescing permanently, and, without a relapse, will recover her former health.

ART. III.—*Observations on Brorchial Respiration.* By J. BLOODGOOD, M. D. (Read before the Chicago Academy of Medicine, Nov. 4th, 1859, and published by request of the Society.)

WHEN the ear or stethoscope is applied over the region of the trachea on a healthy subject, the sound produced by the air in its passage through it is distinctly heard, and is recognized as that of tracheal respiration. At a lower position on the upper part of the chest, this sound undergoes some modification in intensity and pitch, approaching more nearly to that of the vesicular respiratory murmur; into which it soon becomes completely merged.

As the aggregate area of the bronchial ramifications is very considerably greater than that of the main trunk, the air of respiration in penetrating the substance of the lungs must move with a constantly diminishing velocity; as the structure of the bronchial tubes throughout is undoubtedly that best adapted for the transmission of air with the least possible friction; as friction of the air upon the walls of the tubes and cells is the element upon which the respiratory sounds depend, and as we have seen that this friction is constantly diminishing from above downwards by reason of diminished velocity, it follows that in a state of health no sound, whether audible at the surface or not, can be generated in the middle or lower portions of the lungs so great as that of the tracheal respiration itself.

In bronchial inflammation, various sounds which have no existence in healthy respiration, are produced. The mucous rhonchus, caused by the bubbling of air through fluid in the bronchial tubes, easily recognized, varying in character according to the size of the tubes, the position, quantity, and consistency of the fluid; the sibilant rhonchus, probably produced by a circumscribed, somewhat circular constriction of a tube, giving rise to a sound more or less resembling a whistle; and the sonorous rhonchus when the sides of a tube are pressed very nearly together, either by inflammatory thickening, by external pressure, or by the presence of tenacious mucus, causing the air to assume sonorous vibrations similar to those produced by the reed of a hautboy or clarionet, and producing sounds varying from that of a snore to the coo of a pigeon, or the hum of a bee. All of these sounds may, with their many varieties, be readily distinguished when present, though the parenchyma of the lung may retain its normal condition, and sometimes without even the necessity of applying the ear to the walls of the chest.

To enter upon an elaborate description of these rhonchi and their various modifications, with the causes which produce them, is foreign to my present purpose, and would be to reiterate what is familiar to every intelligent physician. I merely wish in this connection to call attention to the fact that when the rational symptoms of bronchial disease exist to any extent, wherever located, whether deeply or superficially, the stethoscope seldom fails to discover it by the physical signs, showing that the tissue of the lung, though an imperfect conductor of sound while in a state of health, is yet a sufficiently good one to transmit to the nearest surface any unnatural sound produced within it.

Dr. John Forbes says, what I believe to be true, that

"When the respiratory sound is perceptible over all its natural sphere and with its natural character, we may be assured that there exists no disease within the chest, which materially affects the lungs; we may be certain, at least, that the air-cells are neither obstructed by fluids poured into them, nor by compression from without. When the natural sound is altered, or no longer exists, we may be assured that disease is present."

This statement is an admission of the fact that any unnatural sound generated anywhere within the chest, either by fluid within the air-cells, or by pressure without, is recognizable at the surface, and if so, any *natural* sound produced there of equal intensity, is recognizable also.

I wish on this occasion to invite especial attention to the form of bronchial or tubular respiration, often heard, because, so far as I have been able to ascertain, it is uniformly attributed to a cause that is insufficient to explain it, one that sometimes exists without the corresponding sound, and is sometimes wanting when the sound itself is present, to wit, the conducting power of hepatized lung which renders audible a sound, usually unheard, but naturally produced by the passage of air to and fro in the bronchial ramifications.

Dr. C. J. B. Williams, in his work on the *Pathology and Diagnosis of Diseases of the Chest*, thus speaks of it :—

"The second stage of peripneumonia is that in which the lungs present that change in the tissue, which is called by Laennec hepatization. This change consists in the gradual effusion of a semi-solid albumen in the interstitial tissues, which, pressing on and obliterating the cavities of air-cells and smaller bronchi, destroys the spongy texture of the lung, and converts it into a more or less solid mass, such a condition of the air-cells precluding any further ingress of air into them, what stethoscopic signs can we have to indicate this stage of inflammation in the living body? Here still a consideration of the physical state of the organ will teach us to expect, *à priori*, the same phenomena that experience has revealed. We have already had occasion to observe that the healthy lung, from its being composed of conductors of very different powers (air, membrane, and liquid), is a bad conductor of sound, and is therefore incapable of transmitting to its surface slight sounds, remote in the interior. But now that the tissue is rendered more uniformly dense by hepatization, it becomes a better conductor and transmits a sound (usually unheard) of the air passing to and fro in the larger bronchial ramifications. This is the *bronchial* or *tubular respiration* of Laennec and Andral, and marks the advance of the second stage of pneumonic inflammation. This sound when once heard cannot be mistaken; it resembles that produced by blowing through a crow's quill, and is frequently so loud as almost to amount to a whistle, acute and defined it forms a remarkable contrast to the dull diffused sigh of vesicular respiration. In other parts of the lung, if the disease is extensive, the breath sound is puerile."

Dr. Forbes, in the article Auscultation, *Cyclopedia of Practical Medicine*, bears similar testimony. He says :—

"It was stated that the peculiar sound produced by the passage of the air in the bronchi, termed bronchial respiration, is not audible in the state of health, except occasionally over the track of the very largest branches; no doubt, this sound always exists in the bronchial tubes scattered throughout the lungs, but in the state of health it is rendered imperceptible, partly on account of the bad conducting powers of the spongy texture of the lungs, and partly from its intermixture with the more general sound caused by the expansion of the air-cells. When, however, the vesicular respiration is obstructed by disease, either by the obstruction or compression of the air-cells (as in inflammation of the lungs or pleura), we then hear distinctly the characteristic sound of bronchial respiration, even in the tubes of middling calibre. This variety of respiration becomes, therefore, an important sign in certain diseases."

Dr. Watson, *Practice of Physic*, says :—

"Sometimes, when this crackling ceases," alluding to the fine crepitation of

the first stage of pneumonia, "the ear applied to the corresponding surface of the chest feels it heave up in inspiration, but catches no sound at all. Much more commonly, however, a new sound reaches the ear. It is not the vesicular rustle, it is not the minute crepitation, but a whiffling sound is audible like that produced by blowing through a quill. This is the sound to which the term bronchial respiration has been given, and the name expresses well the fact. I mentioned before, that in the healthy state we do not hear the air pass through the larger bronchi during inspiration and expiration. The sound, doubtless, is made, but it is obscured and hidden by the smooth rustle of the vesicular breathing which comes from the spongy lung surrounding the large divisions of the bronchi, and intervening between them and the ear; but that spongy structure is now filled up. The hepatized lung permits air to pass through the larger bronchi which are still patent, but it admits none into the vesicles or smaller tubes. It crepitates not when pressed between the thumb and finger; in fact, it is converted into a solid substance, and conducts the sound in the living body as any other solid substance would do, and, therefore, the whiffling, blowing, gusty sound of the breath, as it enters and departs from the larger bronchial tubes which still remain open, is conveyed to the ear and bronchial respiration is heard."

Again—

"The altered condition of the organ gives rise to altered sounds. Instead of the vesicular breathing of health, or the minute crepitation belonging to the first stage of inflammation, we either hear no sound at all, though we feel the chest heave up against our ear, or we hear what I described under the denomination of bronchial respiration; that is to say, a blowing sound which is conveyed to the ear from the larger and still pervious branches of the bronchi through the solid portion of lung around them, and through the solid walls of the chest. This is what the listener hears when the patient breathes, and when he speaks his voice is heard much more resonant than is natural; much more resonant than in the corresponding spot on the opposite side of the chest. We thus become acquainted with two entirely new sounds which are never heard in a healthy state of the lungs, *bronchial respiration* and *bronchial voice*, or *bronchophony*."

Still again—

"But I say sometimes we hear these morbid sounds in the case in question, and sometimes we hear no sound at all during the breathing. How is that? Why do the existence and degree of the bronchial respiration and bronchial voice vary according to the place and extent of the inflammation? These morbid sounds are most plainly marked when the number and size of the bronchial tubes involved in the hepatization are the greater. They are most distinct, therefore, when the inflammation occupies the upper part of the lung or the central parts, what are called the roots of the lungs, and when it extends thence to the surface; but when the lower portions alone are inflamed, or the inflammation is merely superficial or partial, they may not be heard at all. Again, if the inflammation should be so general and complete as to prevent the chest on the affected side from expanding you will in that case hear no bronchial respiration, for the air in the large bronchi must be stagnant; *bronchophony*, however, may remain."

Andral's views differ slightly from those attributed to him in the passage quoted from Dr. Williams. He thinks that the air, being prevented during hepatization from entering the vesicles, strikes with greater force than in healthy respiration against the sides of the tubes, and thus elicits a louder sound.

That hepatization is by far the most frequent cause of this peculiar respiratory sound is doubtless true. Dr. Walshe says: "The tubular form

occurs in perfection in but one condition of lung, that of hepatization; so true is this that tubular and pneumonic breathing may be used as convertible phrases." That hepatization, by rendering the lung a better conductor, enables it to transmit to the surface sounds produced within it, otherwise inaudible, is also true; but that a sound so loud as almost to amount to a whistle, acute and defined, often of greater intensity than any other *naturally* produced within the chest, louder than the various bronchial rhonchi resulting from disease, which, when present to any extent, are always audible, whether the intervening lung be hepatized or not, can be naturally generated there by the passage of air to and fro in the bronchial ramifications, and yet be heard only through the conducting power of hepatized lung requires proof. The theory seems to be entirely inadequate to explain the fact.

All the respired air passes through the trachea where all the conditions that the respiratory organs furnish in health for the production of sound exist in their greatest perfection, and where all the sound produced, which is never so acute as almost to amount to a whistle, can be transmitted to the surface under the most favourable circumstances for being heard without the intervention of lung or any other non-conducting tissue to obstruct its passage. As the conditions necessary for the production of sound are constantly approaching more and more nearly from above downwards to those of the vesicles themselves, so must be also the sound, which can never be so loud below as that heard in the trachea, unless an effect can be greater than the cause that produced it. Every one here knows too well the objects to be accomplished by the function of respiration to believe that the mere production of sound is one of them.

The reason given by Dr. Forbes, in part, and by Dr. Watson, in full, for the absence of bronchial respiration in health, that it is obscured and hidden by the smooth rustle of the vesicular breathing is quite insufficient. No dull diffused sigh can annul a sound of higher pitch and greater intensity, that but for its presence would be heard; a swarm of bees, however large, never drowns the music of the tin pan that is put in requisition for the purpose of hiving them; and supposing the dull sound could conceal the other, what explanation can be given of the fact stated that in hepatization we sometimes hear no sound at all, though we feel the chest heave up against our ear?

How can the chest heave up against the ear unless it expands? How can it expand unless air enters it? How can air enter it without passing through the bronchial tubes! and how pass through those tubes without developing bronchial respiration if the conditions necessary to its development are simply the passage of air and the withdrawal of the confusing vesicular sound, particularly when the only other condition claimed to be necessary, the greater conducting power of hepatized lung is added thereto? All the necessary conditions are present here, but the legitimate effect is

absent. If it be true that bronchial respiration is merely the natural sound of transmission, rendered audible by the absence of the vesicular murmur or the presence of hepatization, one or both, the latter, however slight, if sufficient to destroy the vesicular sound, ought to develop the other in all cases not sufficiently extensive to exclude air from the lung altogether. Dr. Watson tells us that when the inflammation is so general and complete as to prevent the chest on the affected side from expanding, we shall hear no bronchial respiration, the air in the larger bronchi being stagnant, while bronchophony may remain. How can this be if both are naturally produced within the chest and rendered audible by the same cause? The object to be accomplished in speaking is to create sound, the very essence of speech, by creating certain vibrations in the larynx which are communicated to the column of air in the bronchial tubes, transmitted to the surface and recognized there as vocal resonance in health. When rendered more audible by hepatization it is still manifestly only an exaltation of the same natural phenomenon, not an entirely new sound, as Dr. Watson asserts. The object in breathing is the decarbonization of the blood, in which process sound is useless, and forms no part more than what necessarily attends the introduction of air to the blood.

If the sound called bronchial respiration were a necessary part of that process, it should be as readily heard through the hepatized lung of the affected side as the bronchophony, which originates in the same place and is heard.

In healthy respiration the amount of air required for each act of inspiration is proportionate to the space to be filled. When the space is diminished by hepatization the quantity required will also be diminished correspondingly. If the right lung, for instance, is wholly hepatized, the left lung will receive very little, if any, more air at each inspiration than it did in health; the power that causes the air to enter being atmospheric pressure, always uniform, and the expansive force of the respiratory muscles often diminished rather than increased during the inflammatory state. As all the air that enters the lungs is drawn in to fill the vacuum occasioned by the expansion of the vesicles, the draught must be diminished by any degree of hepatization, and cease entirely when the hepatization becomes complete. The theory of Andral, therefore, cannot be true that the air, being prevented from entering the vesicles, strikes with greater force against the sides of the tubes, for all that causes it to enter a tube at all is the demand created for it in the vesicles which the tube supplies.

The instinctive feeling of want of breath is, doubtless, ultimately connected with the presence of black blood in the pulmonary capillaries, and as in inflammation, the rapidity of the circulation is increased, so must be the frequency of the respiratory acts also; but increased frequency does not necessitate increased force. We are still left without an explanation of any respiratory sound more marked than that of puerile respiration.

A theory that fails to explain the facts sought to be accounted for, or that is in conflict with any of them, cannot be true. Some other explanation, therefore, must be sought.

We have seen that in healthy respiration both the friction and consequent sound produced by the passage of the air are constantly diminishing from the trachea downwards, and, very probably, have admired the ingenuity displayed in bringing the very delicate tissue of the lungs into free contact with the atmosphere without endangering its integrity. In disease the conditions are changed; a small bronchial tube that naturally produces little or no appreciable sound may then become a powerful instrument of it, for sound is not determined by the quantity of matter in motion, nor by the velocity alone, but by the resistance offered. The wind whistling through a key-hole is much more noisy than a far larger quantity passing through the open door. The boatswain's whistle is heard above the storm, and the mountain brook babbles boisterously on its way to the deep but silent river. When a portion of lung becomes hepatized the bronchus that supplied it no longer transmits air to the vesicles below, and can, therefore, no longer produce the sound of transmission, great or small; but as the tube itself remains open by reason of the elasticity of its walls, *the air coming from above having no room to enter except by expelling an equal quantity from it, plays around its mouth in passing downwards, and causes the air within to vibrate like the instrument known as the Pandean pipe*, and thus produces the tubular sound which will be high or low, loud or weak, according to the calibre and depth of the pervious portion of the tube, and the force of the respiratory act, which latter will be greater when the upper and middle lobes are affected than when the disease is confined to the lower portions on account of the greater velocity of the air in those parts. When the tube itself becomes filled with solid or liquid matter, or closed by external pressure, this sound will cease.

This theory explains not only the occurrence of bronchial breathing in pneumonic inflammation, but its occasional absence in that disease, by the tubes themselves becoming filled up, and also its presence sometimes in states of disease in which no hepatization exists. In the summer of 1846, I attended a patient suffering with acute bronchitis, in whom I was surprised to find, on one occasion, under the left breast, a loud, distinct, and perfectly characteristic tubular respiration, which did not exist on the previous day, and which disappeared before my visit ended. The case was one of but moderate severity, no other sign or symptom of pulmonary inflammation existed, and the speedy disappearance of this one would have forbidden such a supposition. It could only be attributed to the temporary presence of a pellet of tough mucus, plugging up a tube at some distance below its mouth. The views here presented begin to bear date from the occurrence of this case.

This theory also explains the development of bronchophony at times,

without the corresponding bronchial respiration. Bronchophony being the sound naturally produced in speaking, by the propagation of the vocal vibrations downwards, and known in health as vocal resonance, must necessarily become more distinct by any improvement in the conducting power of the intermediate lung, and may be heard even though an entire side be hepatized, and its tubes be filled up with solid or liquid matter; which latter fact puts an end to bronchial respiration. But if the tubes contain air, it cannot be stagnant, as Dr. Watson asserts, but, communicating freely with the larynx, must receive and transmit the vocal vibrations as perfectly as does the column on the other side, if not more so, being stationary, after the manner of a speaking-tube or stethoscope, in which the column is not stagnant, though it does not move to and fro therein. Under these circumstances, bronchophony will acquire its greatest intensity, and, with all deference to Dr. Watson, bronchial respiration will be produced, because the air cannot pass over the mouth of the bronchus of the diseased side, on its way to the healthy lung, without causing vibrations of the air within it, which will elicit a louder sound than a much larger quantity of air passing freely through the tube would do. This may be easily illustrated with a tube three or four inches long and half an inch in diameter, by blowing forcibly through it to represent the transmission sound, then closing its lower extremity and blowing gently over its mouth, by which a loud tubular sound will be produced with very little effort. Every variety of pitch may be obtained by using a cork for a stopper, and by varying its position in the tube; but it is necessary to its production, that the tube should be perfectly closed. The first case in which my attention was seriously drawn to the value of stethoscopic signs, was one of this kind. The whole right lung was hepatized, and, consequently, immovable, with bronchial respiration most distinct and unequivocal over all its upper part. I have since seen other cases, though fortunately not many, with the same character and the same sign.

Having taken exceptions to Dr. Watson's statement that, "when the entire lung is hepatized, you will have no bronchial respiration, for the air in the larger bronchi must be stagnant," I will quote, in support of my position, from a case recorded in *Andral's Medical Clinic*—"Diseases of the Chest"—than which no higher authority can be found:—

"CASE II. Post-mortem. Red hepatization of the three lobes of the right lung." "This is the first instance we meet of hepatization of the entire of a lung. When the patient entered La Charité, the signs furnished by auscultation and percussion announced the existence of this hepatization, not yet complete, in the middle and lower lobes; the upper lobe was, as yet, but engorged. The following days, it became rapidly hepatized; but, in this lobe, the hepatization was announced by the *bronchial respiration*, whilst in the other lobes—whose alteration, however, seemed to be the same—this characteristic sound was not heard. We think it right, carefully to note these differences of signs afforded by identical lesions,

in order that we may be accustomed to recognize these lesions whatever may be the shades announcing them."

This case demonstrates, what any number of negative facts cannot disprove, that the transmission theory is not only unnecessary, but wholly inadequate to explain the existence of bronchial respiration, for it occurs here under circumstances which would render its occurrence impossible, if the theory were true, and which, Dr. Watson says—more in deference to theory than to fact—do render it impossible. The lung is solid and immovable, no air is transmitted through its tubes, and no sound of transmission can by possibility be produced, yet the bronchial respiration is distinctly heard, and must, therefore, be due to a different course.

The phenomena of cavernous respiration and pectoriloquy illustrate this subject. Tubercle is deposited in the capillary structure of the lungs, and does not usually obstruct the bronchial branches, except by external pressure. When it softens and is discharged, an ulcerated cavity remains, the walls of which become sufficiently indurated by the deposition of lymph to prevent collapse and render them impervious to air, except through the opening into the adjoining bronchus, through which the tuberculous matter has been discharged. Here the conditions are essentially the same as in the former case. The air does not circulate through the cavern, but vibrates within it, and develops cavernous respiration and also pectoriloquy —more or less perfect, according to the form and size of the cavity—by *consonating* with the vocal vibrations in the larynx. A small tubercular cavity, with a wide mouth, might generate only bronchial respiration (from which the cavernous differs but in degree) and bronchophony; while a dilated bronchus, with a contracted mouth, might equally well develop cavernous respiration and pectoriloquy, though such combinations may rarely occur. In hepatization of an entire lung, the main bronchus of that side becomes, in effect, a cavern, from which it differs only in form, and *generates*, by the vibratory motion communicated to the air within it by the passing air without, the true tubular respiration, which, we have seen, could have had no existence during a state of health.

I grant that, in certain cases, the natural sound of transmission may become audible through the intervention of hepatized lung—when, for example, a portion of the upper or middle lobe, lying between the tubes that supply the still healthy lower lobe and the surface, is in that state—but the sound will be no louder than the vesicular murmur that obscured it, and, in a majority of cases, will be disguised by, or lost in, the louder bronchial sound produced in the tube that formerly supplied the hepatized part. When the lower lobes are exclusively the seat of hepatization, the transmission theory furnishes no explanation whatever of the bronchial breathing. The tubes that supply them transmit no air, and, according to this theory, must be silent; while those above are still surrounded by healthy

tissue, and must transmit the natural, or, at most, the puerile sound. Indeed, if this theory be true, I see not how the bronchial respiration can ever be developed, unless the lower portions remain pervious while hepatization exists above; and, as inflammation and its consequences are much more frequent in the lower than in the upper lobes, bronchial respiration should be the exception rather than the rule, as it manifestly is.

The theory herein presented is sufficient to account for its presence whenever it exists, and for its absence when it is wanting; for its greater frequency and intensity when inflammation affects the upper lobes than when confined to the lower portions; and, if true, will justify sometimes a more favourable prognosis in cases of bronchial respiration than would be warranted if hepatization were present.

ART. IV.—*On the Seat of the Vesicating Principle of Lytta Vittata.* By JOSEPH LEIDY, M. D., Prof. of Anat. in the Univ. of Penna. (Read before the Biological Department of the Academy of Natural Sciences, September 5, 1859.) With a wood-cut.

IT often occurred to me that the vesicating principle of the blistering fly, *Cantharis, Lytta*, etc., was the product of some special organ in the insect, and was not diffused through the body of the animal. In polyps, bees, wasps, mosquitos, etc., we find the irritating agencies or poisons produced by particular cells and glands, not to mention the poisons and odorous principles found in special organs of higher animals. A few days since, observing numerous individuals of Lytta vittata feeding on Amaranthus albus, I collected a number, and at a leisure moment experimented with the different parts of the animal to ascertain if the vesicating principle was confined to some special part of the insect. I did not then recollect of having read of others who had performed similar experiments with the same object, but on examination, find in Pereira, under the head of Cantharis vesicatoria, the following remarks.

"The active and odorous principles of cantharides reside principally in the sexual organs of the animals. Both Farines and Zier tell us that the soft contain more active matter than the hard parts. It appears, also, that the posterior is much more acrid than the anterior portion of the body; and Zier says the ovaries are particularly rich in this active matter."

Though I have long been familiar with the researches of Audouin on the anatomy of the Spanish fly, I remembered that he had made no mention of the source of irritating power in the insect. The anatomy of Lytta vittata is so nearly like that of the Cantharis vesicatoria as described by Audouin, that I shall say nothing about it except in the way of reference.

Without examining the work of Farines and Zier, I will detail the results of my experiments on the Lytta vittata, and leave it to others to compare them with the experiments of the former authors.

The Lytta vittata appears not to possess the peculiar mice-like odorous principle of the cantharis vesicatoria, at least I have not observed it in living or fresh dead specimens, nor in specimens dried and long preserved.

When the Lytta is caught, it exudes a clear yellow liquid, usually in one or two drops from a corresponding number of the knees, though smaller drops are not unfrequently seen to appear at other joints of the legs. This liquid appears to be the blood of the animal, for it is in all its physico-microscopical characters like this fluid obtained from any part of the body. If an elytrum is cut across, one or two drops of the same liquid exudes from vessels of the cut margin. The liquid forms a fibrinous coagulum, and contains colorless corpuscles, like the blood of insects ordinarily. The exudation occurring on the capture of the Lytta, I suspect to be the result of voluntary rupture of the parts, which is not extraordinary when we take into consideration the ease with which insects will sacrifice a leg. Be it as it may, the yellow liquid from any part of the body vesicates. Portions voluntarily exuded on the capture of the insect, others from the cut borders of the elytra, from cut extremities of the legs, and from the head, imbibed by separate portions of bibulous paper produced upon the inner side of my forearm a corresponding number of blisters. As the nettle among plants, and the larva of the egger moth have stinging hairs, I was led to try those of the Lytta. A quantity scraped from the elytra and other parts were mixed with cerate and applied to my forearm. They proved to be inert.

Half a dozen elytra cut into fragments and mingled with cerate produced a blister. This was the result of the contained yellow liquid or blood, for a nearly entire elytrum vesicated only at the point of contact of the cut border.

The intestinal canal with its leafy contents produced no effect. The muscles of the thorax, and the rete adiposa, or fatty matter common in insects, separately applied to the forearm produced no effect. The testicle and epididymis of the male likewise were inert; as were also two long tortuous accessory glands of the generative apparatus in the same sex. Two other accessory glands of the generative apparatus in the male, equally long and tortuous with those just indicated, are distended with an opaque white granular matter. This matter, mingled with cerate, was found to be highly vesicant. Viewed with the microscope it seems to be a consistent fat-like substance; appearing as spherules of various sizes with dark outlines, fainter concentric lines, and

others radiating, as represented in the figure. The spherules recalled to mind the appearance of the crystalline lens with its concentric layers and radiating fibres.

The ovary vesicated. This result also was obtained from the isolated eggs, crushed and mingled with cerate. A large pyriform sac attached to the generative apparatus of the female, contains a yellow sebaceous-like matter, together with a mass of hard opaque white substance. The former material appears to consist of spermatic matter, epithelial cells, and an albuminous substance; and it is inert as a vesicant. The mass of hard white substance is composed of the same fat-like spherules above described as existing in accessory glands of the male generative apparatus. I have occasionally observed it to be absent, and suspect that together with the other contents of the pyriform sac it is derived from the male. Mingled with cerate, I found it also to be a vesicant. My friend, Dr. Darrach, also applied portions of the two different substances, from the pyriform sac of the female generative apparatus, to his forearm. The fatty-like spherules alone vesicated.

Thus, as a result of the experiments above briefly detailed, the vesicating principle of Lytta vittata appears to belong to the blood, the peculiar fatty substance of certain accessory glands of the generative apparatus, and to the eggs.

ART. V.—*Remarks on the State of the Capillary Bloodvessels in Inflammation.* By JOHN H. PACKARD, M. D., of Philadelphia.

OF all the points involved in the discussion of that most complex process, inflammation, none, perhaps, has been so warmly disputed, to so little purpose, as that above mentioned. Some pathologists have claimed for the capillaries a contractile power, which they say is exerted for a time, leaving them subsequently in a state of debility; others deny this contractile power, asserting that the vessels are from the very outset enfeebled. Others again have recourse to the idea of an active dilatation, which they attribute to an alteration in the electrical state of the atoms constituting the walls of the capillaries concerned.

Microscopic examination has often been resorted to for the purpose of clearing up this matter. But observers have not arrived at altogether uniform conclusions as to what is seen when artificial stimuli are applied to the living tissues. All agree that there is a period of dilatation of the capillary walls, and that there is a stagnation of the blood-corpuscles, when inflammation is thus excited; some, however, assert that the dilatation is preceded by contraction, while others see only the former phenomenon.

So familiar to the medical reader are the accepted authorities on this subject, that the foregoing brief statements may suffice, without the recapitulation of well-known names. These writers have, with respect be it spoken, too much neglected the intimate blending of healthy and morbid states, so justly insisted upon by Mr. Paget.[1]

What, then, is the function of the capillary bloodvessels in health? From their anatomy it is a natural inference that they are mere channels for the passage of the blood; that they dilate in obedience to pressure from within, and contract in obedience to pressure from without. Thus in the web of the frog's foot they are seen only as defining the current of blood; their structure seems to be as simple as that of a cell-wall. And judging from what we are told as to their development by the coalescence of cells, they would seem, in fact, to be composed of the so-called structureless, basement, or limitary membrane.

Mr. Lister has indeed described, in the *Transactions of the Royal Society of Edinburgh*,[2] non-striated muscular fibres arranged spirally around some of the smaller arteries of the frog's foot. But this circumstance does not affect the question now under consideration, any more than does the known presence of elastic fibres, and of this form of muscular fibre in the coats of the arteries generally. Nor is there any evidence that there is such a thing in the animal economy as an active dilatation. Muscles always contract, when they are used; another set of muscles is provided, to contract in their turn, when the opposite motion is to be made.

Hence, in ordinary nutrition, the only use of a contractile power in the capillaries would be in preventing undue distension of them by the blood sent from the heart; for the continuous character of the current in them is wholly unlike the peristaltic motion which would be produced, were such a contractile power to cause or to share in causing the passage of the blood.

According to the physiology of the present day, the power by which the blood is moved along in the capillary vessels resides in the tissues themselves; that is, in the structures which occupy the interspaces of the capillary networks throughout the body. Oxygen and nutritive material are wanted by these structures; hence they attract the blood-mass nearest them. Having exhausted this portion of blood so far as they are able, they, as it were, reach out for that which succeeds it, and so on; the new supply pushing that which has become more or less effete before it.

The different parts of the body are variously endowed with vascularity, obviously because they have their several *rôles* to play in the economy, and are fitted accordingly. In like manner the degrees of susceptibility to stimulation which they manifest vary greatly; some are liable to an incessant ebb and flow of blood in their alternations of rest and activity, others are almost or quite uniform in the quantity of blood they contain. To

[1] Lectures on Surgical Pathology, Am. ed., p. 192.

[2] Vol. xxi. p. 549.

account for these fluctuations, I look upon the supposition of a vital force as absolutely necessary; and this vital force seems to me to be, in great measure at least, under the control of the nervous system. Comparative physiology shows that there is a difference in this respect between the higher and lower orders of living beings; in man the vitality of the whole animal is to a great degree centralized in the brain and spinal marrow, and especially in the former.

Now when a function is performed, either with or without volition, it is the result of an increased activity of the vital force residing in the part. This increased activity may arise from a mechanical or chemical stimulus, or from some influence less readily explicable, such as an electric shock, or an alteration of temperature; or it may be to all appearance spontaneous. In every case but the last named, the principle of reflex action is of great importance in tracing out the chain of phenomena.

An increased afflux of blood invariably ensues upon an increase of the activity of any part, because this increase of activity involves waste of substance, and this again must be made good. It is, therefore, because the intervascular structures want blood, not because the capillaries are either enfeebled or actively dilated, that a part receives more blood under these circumstances.

Now, the recognizable causes of inflammation are either stimuli, primarily increasing the activity of function of the part concerned; or depressing agents, inducing the same effect secondarily. What is the precise mode in which either influence is exerted, whether directly upon the tissues, or indirectly through the nerves, is not yet known; perhaps it will always remain a mystery. But there is in every case a stage of exaltation of the life-actions of the part; and it seems to me that herein is to be found the true explanation of the distension of the capillary bloodvessels. The contraction sometimes observed upon the application of stimuli, especially of smart mechanical irritation, may, I think, be easily accounted for upon this theory, and thus an apparent discrepancy between different statements may be reconciled. Such an irritation may induce a state of local collapse, like that which affects the system at large after severe injuries; so that the paralyzed tissues cease for the moment to attract blood, and the calibre of the vessels diminishes because their contents are lessened. The subsequent reaction goes on just as the primary excitement does in other cases.

If the inflammation continues, the over-stimulated tissues are unable to dispose of the blood they have acquired, which, therefore, stagnates, blocking up the vessels; and as the general circulation goes on, the fresh blood which comes to the part must find a passage elsewhere. Hence ensues a crowding of the vessels in the neighbourhood of the focus, and hence the gradations from the point of greatest obstruction outwards to the healthy parts around.

Should this stagnation be absolute, and should it last beyond a certain

length of time, the tissues will die for want of oxygen; the cutting off of their supply of nutriment may also share in causing their death. Such is probably the rationale of many cases in which an inflamed part becomes gangrenous.

But stagnation does not always ensue in inflammation, although the current of blood may be very much slackened; in chronic inflammation, without suppuration, it is probable that this latter condition is one of the chief constituent phenomena. Such a state of things might more properly be termed permanent congestion.

Whether the current be merely rendered slower, or checked altogether, it is easy to see why throbbing should take place in the arteries supplying the part affected, and why more blood should flow from an opening in the corresponding vein. A greater resistance than usual is encountered by the arterial wave, which, therefore, spends its force upon the walls confining it, and is more readily perceptible. The other circumstance alluded to affords no evidence that more blood passes through the affected part in any given time, but rather that the contrary is the case; for the same quantity of blood being forced into a less capacious receiver, will necessarily be more rapidly forced out wherever the resistance is taken away.

Any application, such as uniform pressure, which restores the blood-current to its normal unembarrassed condition, will tend to lessen inflammation; and the combination with this of an obtunding influence upon the sensitiveness of the nerves of the part, will add greatly to the remedial effect. Thus, astringents generally are employed with the former view, and also, the nitrate of silver especially, with the latter. Cold, also, and moderate warmth, accomplish both these objects. None of these remedies act upon the vessels themselves, but upon the structures occupying their interstices, according to the idea which I have maintained.

The attraction theory of the capillary circulation was first developed by Haller, about eighty years ago. It has been, as already stated, adopted by the physiologists of our time. And yet pathologists seem to have nearly lost sight of it. The late Dr. Alison is the only writer, so far as I am aware, who has made it the basis of a theory of inflammation. He uses the following language:[1]—

"From all these facts we think ourselves justified in inferring, that inflammation consists essentially in a local increase of a vital property of attraction existing among the particles of the blood, and between them and the surrounding textures, and with which other vital properties are connected, and simultaneously excited. That the proximate cause of inflammation, although affecting the constitution of the blood, does not reside in blood only, but primarily in the agency on the blood of the solids through which it passes in the capillary vessels, appears clearly from the limitation of the disease to a certain locality in the body, from the fact of its easy reproduction, for a long time, or for life, in the vessels which have once been the seat of it, and from other facts to be mentioned as to inflammatory effusions."

[1] Outlines of Pathology and Practice, Am. edition, 1844, p. 84.

I cannot but regard it as singular, that this view has excited so little attention; or, rather, that it has not suggested itself to others who have studied the subject. When Dr. Alison wrote, the phenomena of reflex action had not been so clearly made out as they have since been, nor had the question as to the existence of a vital force assumed its present aspect. I may, perhaps, be excused for saying that the views expressed in this paper had been fully developed in my own mind, before the passage above quoted met my eye.

We must not hope to understand *life*. All attempts to disguise our ignorance of this mystery, by the rejection of the term "vital force" and its correlatives, are in reality unphilosophical. Certain it is, that vitality is manifested by organized matter, according to laws which the creative Power has enacted, and which we ascertain by inductive reasoning. Is it more rational to assign to this vitality a vague and undetermined haunting of the living tissue, or to regard it as the attribute of the organic cell? But if each cell has its life, with all the constituent phenomena of that process, the idea of an attraction between it and the living blood in the vessels may be as easily accepted as may that of the attraction of gravitation.

Now, assuming such an attraction, and bearing in mind the well-known influence of the nerves of a part over its nutrition, we have an explanation of the mode in which external agents may give rise to inflammation. This process, however, seems often to occur spontaneously, or at least to result from influences which we are unable to detect. When such is the case, we may still infer that it is the vital activity of the part which is increased, primarily or secondarily, with or without the agency of the nerves distributed to it; for there is no evidence of anything peculiar here, except in the initiation of the process. Thus, when a man takes cold, as the phrase is, the activity of the skin is lessened by a depressing influence from without; and a transference of that activity takes place to the mucous membrane of the bronchial tubes, or to that of the nose. Now, especially if the nerve-centres be regarded as the source of the stimulus which keeps up the vital activity of each part, the nasal or bronchial mucous membrane will in the case supposed receive an amount of stimulus quite out of proportion to its extent, and hence will be over-excited, and the rest of the process will go on as before explained.

When, as sometimes happens, inflammation seems to be set up in a part spontaneously; when no cause, either immediate or remote, can be assigned for its occurrence, we may regard it as probably due still to some agent which gives rise, primarily or secondarily, to exaltation of the vital activity of the part. And in neither of the cases first stated does it seem as if there were any greater reason for believing that the walls of the bloodvessels take any active part, than in the frog's foot, where the eye can trace the progress of things.

If in the foregoing remarks I have succeeded in clearly stating the idea

which is present to my own mind, they will not render me amenable to the charge of solidism. Such a view seems to me to present, as Virchow has said with reference to his cellular pathology,[1] "the true combination of solidism and humoralism."

ART. VI.—*Case of Successful Operation for Vesico-Vaginal Fistula.* By WASHINGTON L. ATLEE, M. D., of Philadelphia. (With eight wood-cuts.)

FOR reasons which will be apparent in the remarks following the report of this case, I have thought proper to precede the details of the operation by extracts from a letter written, at my solicitation, by Dr. J. C. Weatherby, of Clarksboro', N. J., giving a lucid and truthful history of the case before it came into my hands. This history is particularly valuable, as showing: 1st, that vesico-vaginal fistula may occur in females, who have previously had numerous children without any difficulty, whose pelves are well formed and amply capacious, and whose labours are managed by skilful accoucheurs; 2d, that the injury, although coincident with parturition, may not be the effect of it, nor dependent upon any manipulation at that time; and 3d, that it really may arise from a condition not heretofore suspected of being capable of producing such a result, and that, too, prior to the commencement of labour. Dr. Weatherby writes:—

"I was called to Mrs. S. on the morning of the 2d of June, about 4 o'clock. Found her in labour. Pains moderate, os uteri dilated to the size of a half dollar, and waters discharged. A tedious case anticipated. The head was high in the pelvis. I felt no anxiety about the case. Spent an hour or two with Mr. S. down stairs. Made another examination and found the os more dilated and head lower down. Observed nothing to cause any apprehension for her safety. About 9 o'clock I found the os well dilated and the head pressing very hard on the symphysis pubis, and not disposed to come down as fast as I should have liked. Her pains were at this time very trifling, and I gave a portion of secale cornutum. I asked when she had last voided urine. She answered that she had a short time previously, and also informed me that for some two or three days she had been troubled with partial incontinence, expressed in her own way.

"The pains now assumed a more formidable character, and in fact she complained so bitterly of them, that I gave her about two grains of opium in pill. The pill had the desired effect, but pains continued forcible, head lower and os uteri entirely over it.

"About this time, when the head was entering the inferior straight, my trouble commenced. The head seemed to be fixed permanently. Pains forcible and ineffectual. This was about 10 o'clock. Resorted to the use of forceps, the sixth or seventh time in twenty-two years' practice, and this

[1] Archiv, April, 1855.

my first failure. Sent for Dr. Clark. Tried to evacuate the bladder. Dr. Clark arrived about 1 P. M.

"After I sent for him do not think the head descended a particle. Told him she had not passed urine for two or three hours. I could find none. Suggested that he should try. He did so. Found no water. I tried to use his forceps, but they pertinaciously followed the old path of my own. He tried, and again the same direction was taken and they refused to lock. At this time he passed the female blade laterally over the head, and they were made to lock.

"The head was soon delivered. The shoulders seemed now as immovably fixed as the head had been, and not until a napkin had been placed around the neck and force almost sufficient to sever the head was used, with considerable manipulation, could the child be delivered. This was about 2 P. M.

"After I delivered the placenta some time, I again introduced the catheter, thinking that should there be any urine present, it might, with the subsequent accumulation, be a source of trouble to her before I should see her the next morning, and that certainly it would then have to be drawn by the catheter, after so hard and difficult a labour. I found no water.

"3d June, I visited her and found her a great deal more comfortable than I had expected. She said she had passed water all right. I was surprised at that, for often after deliveries not half so irritating to the parts have I had to use the catheter next day. Great tumefaction of the labiæ. Ordered rags wet with water to be constantly applied. Gave medicine to move the bowels.

"*4th.* Visited her again, and found her more comfortable; tumefaction in a great measure subsided. Complained of the frequency of her stools. Gave a Dover's powder. Had passed water all right. Had pain in one leg and foot, which I ordered to be rubbed with a stimulant liniment which they had on hand and had been using on their own responsibility. She said she felt smart, to use her own expression, and had no pain or soreness, except in the leg.

"On the 5th, and for two or three days after, I watched that leg. Heard no complaint about the water till some ten days after, when I called as I was passing, and then she complained of the incontinence. This was the 14th of June. I know not how many days she had so been troubled, and supposing it might be something of the same kind she had complained of before she was confined, and that it might be a paralytic affection of the sphincter vesicæ, I ordered tincture of cantharides thirty drops three times a day. In the evening of the same day I was sent for to draw her water with the catheter. I proceeded to use the instrument, and to my astonishment and utter horror I discovered the fistula, and immediately informed her of its existence. The first time I saw Dr. Clark I told him, and he was as much astonished as myself. * * *

"I ought to have stated that such a degree of anterior obliquity of the womb I never saw before. It arose at a right angle with the line of the body just above the pubis, and to that cause I attributed the great difficulty of delivery.

"The fistula, I *suppose*, was produced by a slough caused by pressure of the child's head on the bones of the pubis.

"Now this I know, that neither blade of the forceps in my own or Dr. Clark's hands was in the region of the bladder, and had no need of being. The use of one blade as a vectis I did not attempt, nor never did in any

case. I further believe that forceps blades, with the caution and non-forcible manner I introduce them, would not cut the bladder if they came in contact with it. If I ever should do it, I think I should know it at the time. I should think it would be the cause of great pain, yet in using the forceps no pain or hurt in that region was complained of. There was no hemorrhage at any time prior to delivery. * * *

"I do not believe that the fistula in the bladder of Mrs. S. occurred through any negligence or want of skill on the part of Dr. Clark or myself, but that it was one of those unfortunate accidents, which are not regarded so much a complication of labour as one of its sequelæ, and liable to happen in hands the most skilful."

In a subsequent letter Dr. Weatherby writes: "With reference to the position of the child's head, I believe the presentation at first was the third of Dewees, subsequently changed by my manipulation to the first. I must confess that to me obscurity attaches to that case all through. The head seemed to be jammed and immovable, even after it attained the last position. And so with the shoulders, as I mentioned in my first letter.

"My forceps are Baudelocque's with Hodge's improvement. They are the long forceps; have served my purpose well several times formerly; and I have used them about once in three hundred cases, with uniform satisfaction till the last."

A statement was also drawn up for me by C. F. Clark, M. D., of Woodbury, N. J., sustaining in every particular the above excellent history of the case.

Mrs. S. is the mother of nine children, the last one, above referred to, having been born dead. No difficulties occurred in any of the previous labours. She is a short, stout, well-built, and healthy woman, and has menstruated regularly since the birth of her last child.

August 1, 1859. She was brought to my office, where I made a digital examination, and ascertained the existence of a very large opening in the vesico-vaginal wall. As a menstrual period was approaching, I requested her to return home and come again to the city immediately after menstruation had ceased, and submit to the operation.

19*th*. Mrs. S. having returned to the city, I made a more careful examination of the parts, assisted by Dr. T. M. Drysdale. The patient having been placed in a proper position, and the rectum sustained by Sim's speculum, the opening was readily inspected by the eye. It was at least one inch and a quarter in diameter in all directions, making almost a perfect circle. Through the opening was seen protruding the red, rugose mucous coat of the bladder. The whole trigonum vesicæ seems to have been destroyed. The lower margin of the opening is about one inch and a half from the orifice of the urethra. The vagina, throughout its whole extent, is very much contracted, particularly above, where it forms a distinct ring, through which the healthy os tincæ can be seen. In consequence of the narrowness of the vagina, the edges of the speculum and the lateral margins of the fistula were closely in apposition.

There being still some evidences of menstruation, I postponed the opera-

tion for a few days, and as the narrow vagina afforded so little space for manipulation, I attempted its dilatation by means of compressed sponge. The too rapid expansion of this sponge, in consequence of the copious supply of urine, produced great suffering, and the patient could tolerate its pressure only for a few hours. Still something was accomplished, even by its limited action.

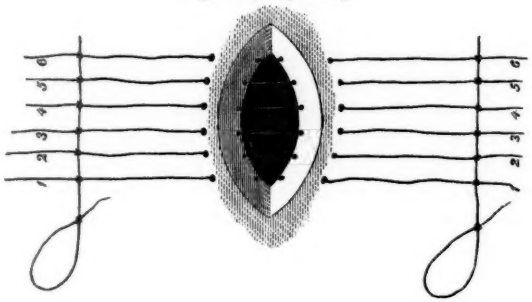
22*d*. This day was fixed for the operation. Yesterday morning the patient took a dose of castor oil, which operated well during the day, and on going to bed she took twenty drops of elixir of opium. This morning, about one hour before the operation, she took thirty drops more. Chloroform was not employed. My assistants were Drs. T. M. Drysdale, George Spackman, Wm. B. Atkinson, and Thomas Ridgeley, of Philadelphia, and C. F. Clark, of Woodbury.

The patient was placed upon a table opposite a window admitting the bright rays of the sun, and occupied a position on her elbows and knees. The rectum was well elevated by Sim's speculum, the fistula was brought into good view, and the whole vagina was completely illuminated by the sunlight. The edge of the fistula was now transfixed by means of a spear-shaped bistoury, the point entering the vaginal mucous membrane about three-eighths of an inch below it and coming out at the vesical border. The knife was now carried carefully and slowly to the right and left until a complete circle of the above width was cut away, thus securing a raw surface all round the edge of the fistula by one sweep of the knife. A section of one-fourth of the circle was easily made by the bistoury alone; but in order to complete the cut without difficulty, the flap already made was supported by an artery forceps, so as to keep the parts in a proper state of tension. The portion removed left no doubt that the vaginal surface of the fistula was well denuded. The parts were now cleansed of blood by small soft sponges, and the fibrous structure of the internal termination of the urethra was plainly visible traversing the lower portion of the wound. As this completed the first stage of the operation, I now allowed the patient to rest upon her side until I prepared my arrangements for the next procedure.

Having made the necessary preparations, the patient was placed again in position, the parts carefully cleansed of clotted blood, and the edges of the wound well examined to see that there was a wide strip of raw surface, I proceeded with the operation. A Sim's needle, armed with annealed iron wire, No. 32, was passed into the right side of the vagina, entering about half an inch below the edge of the incision, passing through close to the mucous coat of the bladder, and emerging near to the vesical edge of the wound; then again entering at a similar point opposite in the upper lip, and finally emerging at the vaginal surface about half an inch above the upper border of the cut. So soon as the point of the needle was seen escaping above, it was caught with the blunt hook and held until the needle

holder was detached below. Then seizing the point of the needle with the forceps, it, with the wire suture, was readily withdrawn. The suture was about eighteen inches long, and half of it was drawn out. To prevent the wire from cutting the mucous membrane it was drawn over the director invented by Dr. Bozeman. Knowing that with such an extensive wound I should have to use several sutures, whose numerous ends would make confusion in the future steps of the operation, I adopted a very simple and effectual means of avoiding this. The two ends of the wire were each tied with separate threads, and held out of the way. Afterwards five more sutures were introduced precisely in the same way, making six sutures in all, about one-fourth of an inch apart, and as the ends of each suture were brought out of the vagina they were secured by the thread in their regular order. This, the second stage of the operation, is represented by Fig. 1.

Fig. 1. [*Half size.*]



An inspection of this diagram will at once show that this contrivance prevented the tangling of the wires, and enabled me, at the same time, to identify each wire as it was needed. The result, also, proved that the arrangement was an admirable one. It facilitated many of our future movements, and entirely obviated the necessity of raking out each wire from its fellows. A loop was tied on one end of each thread, so as to secure us from every possible mistake.

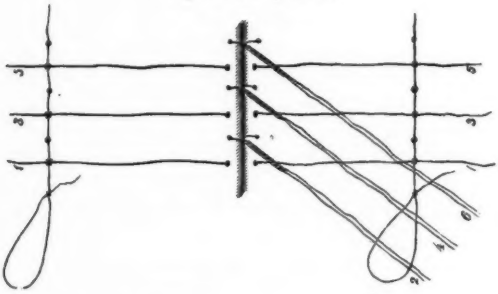
This part of the operation was tedious and difficult. The walls of the bladder, with its turgid mucous coat, crowded through the fistula, and continually interfered with the free use of the needles. But by the aid of an assistant, who kept the protruding parts out of the way of the points of the needles with the blunt hook, I succeeded in satisfactorily locating all the sutures.

The third stage of the operation, or coaptation of the edges of the wound, was next in order, and the mode adopted, together with the plan of securing the sutures, possesses certain peculiarities, which I flatter myself are real advantages over previous methods. The two ends of suture No. 2 were drawn from the knots in the threads, and passed through Dr. Coghill's

twister, a most admirable and ingenious little instrument, which was slipped over the wires down to the wound, and after the lips of the latter were accurately adjusted by the fingers, a few turns were given to it, and thus the suture was rapidly and neatly secured. The same was done with the ends of wire No. 4, and also those of No. 6, being every alternate suture. This brought the whole wound most beautifully together, and the lips in exact apposition. This instrument, Fig. 2, is the invention of Dr. Coghill, and is described by Dr. Simpson, in his *Clinical Lectures on Diseases of Women*, in *London Med. Gaz.*, Jan. 8, 1859; (see also *Med. News and Library*, Feb. 1859), as follows: It consists essentially of two extremely short and very fine tubes fixed on the end of a steel rod, which is used for finally tightening and fixing the sutures. One end of a wire having been brought through one of the small tubes of this twister, and the other end through the other, the instrument is pushed down close to the lower bar of the splint; and then both ends of the suture having been drawn quite tight, so as to make the splint compress and consolidate the parts, a turn of the twister suffices to fix them there. The figure represents the instrument of full size, in the act of twisting the wire passed through the two terminal eyes. After having secured these three wires by the twister, I omitted cutting them off in order to accomplish an object in a subsequent part of the operation. All present now inspected the fistula, and saw how completely it was closed and secured, although only every alternate suture was tied. Fig. 3

Fig. 2.

Fig. 3. [*Half size.*]



represents this stage of the operation, and also explains how we may be certain of the proper and unalterable adjustment of the wound before the application of the button.

The next step was to attach the button or splint, and as this part of the process, in my opinion, is a decided improvement upon Dr. Bozeman's, both

in the peculiar form of the button and its mode of attachment, I wish to call particular attention to it. Fig. 4 represents the size and shape of the button, which I constructed myself in a few moments. It is made of rolled lead of the thickness of two and a half pounds to the square foot. It has a fenestrum along its centre, proportioned to the size of the fistula, the under surface of which fenestrum, or that part of the plate in contact with the wound, is bevelled off, as seen in the end view of the figure. On each side are arranged parallel and equidistant holes.

Fig. 4.

The two ends of suture No. 1 were now drawn from the knots in the threads, and successively passed through their appropriate holes No. 1 of the button. The twisted suture No. 2 was next passed through the fenestrum. The ends of suture No. 3 through holes No. 3. The twisted suture No. 4 through the fenestrum. No. 5 through holes No. 5, and No. 6 through the fenestrum. The ends of Nos. 1, 3, and 5, in the order mentioned, were then passed through the twister, which was slipped down over the wires to the plate, and, after adjusting the latter accurately, this was firmly secured by a few turns of the instrument. The twister was removed and the wires cut off close to the twist, and the projecting end pressed down against the plate. The remaining sutures, Nos. 2, 4, and 6, emerging from the fenestrum, were afterwards perfectly secured, by passing over them perforated shot, which, having been pushed down to the button, were compressed upon the wires by a forceps, and the wires cut off close. Fig. 5 represents the several stages of this part of the operation, and Fig. 6 the appearance of the parts when completed.

Fig. 5.

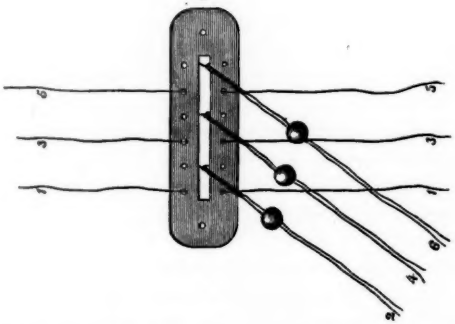
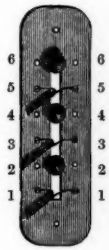


Fig. 6.



The patient, who had been on the table just one hour, was now placed in bed on her back, a urinal placed under her, and a Sim's catheter introduced, which gave exit to about half an ounce of urine. She complained

of no pain during the operation, and assured me that she suffered less at my hands than she did from the irritation caused by the constant stillicidium of urine.

September 1, 1859. Every day since the operation the patient has taken a dose of elixir of opium morning and evening, for the purpose of keeping the bowels quiet, and enabling her to bear with less fatigue the constant confinement in the same position. The catheter was also removed twice a day and cleansed. The deposit of the triple phosphate upon the catheter was very rapid, and by the time I omitted its use its calibre was almost closed by the incrustation, notwithstanding I had cleansed it out twice a day with hot water and a piece of whalebone. The vagina was also syringed out daily with lukewarm water, the patient at the same time being on the bed-pan.

This morning, on making my visit, she said she had a great desire to pass urine, which she had not experienced since her parturition, and upon examining the catheter I was greatly alarmed to find it nearly out of the urethra, and entirely out of the bladder. There was besides no water in the urinal, which had been emptied two and a half hours before. Although previous to this time not a drop of urine had leaked away, I was apprehensive that this retention of urine might, by distending the bladder, disturb the tender adhesions and thus frustrate the object of the operation. Upon replacing the catheter about four ounces of urine came away in a strong stream. Not a drop, however, escaped through the fistula.

With the assistance of Dr. Drysdale I now proceeded to remove the button and several of the sutures. The patient was again placed upon her elbows and knees before a window admitting the sunlight, so as to furnish a good view of the parts upon elevating the rectum with the speculum. The button was in perfect apposition with the soft parts beneath, and as firm as it was when first placed there. With Sim's scissors I first cut sutures 1, 3, and 5, between the twist and the plate, and seizing the twist with the forceps I withdrew them with ease. Next, I cut off the three shot which clamped the sutures 2, 4, and 6, coming through the fenestrum, and yet, notwithstanding all the original support to the plate was now removed, the plate itself still clung to the soft parts. On attempting to lift it away with the forceps I found it required more force than I thought proper to use, and upon examining into the cause of this resistance I discovered that the fenestrum was occupied by the cicatrix, and that this had to be carefully dislodged by the blunt hook before the plate could be separated. After removing the plate, and inspecting the wound, it exhibited a most beautiful aspect. It was firmly healed from one end to the other, and wholly free from any appearance of irritation. The three sutures, 2, 4, and 6, although freed from their clamps, still retained their twist, and were allowed to remain in position as a further protection to the cicatrix. The patient was again placed in bed, and the catheter introduced.

Although the plate was straight, as in Fig. 4, when it was applied, it had assumed the regular curvature of the parts at the time of its removal. Its shape is represented by Fig. 7. This I consider gives to lead a decided advantage over every other metal, and over the wire splint of Professor Simpson, as, while it protects the wound with the greatest certainty, its yielding nature prevents it injuring any of the soft parts.

Fig. 7.

4th. Not a drop of urine has escaped since the removal of the plate. The catheter has still been removed twice a day and cleansed. To-day I placed the patient again on her knees before a good light, and, with the assistance of Dr. Drysdale, proceeded to remove the three remaining sutures. They were pretty well buried in the tissues, and somewhat difficult to nip with the scissors. They were, however, finally satisfactorily removed, leaving the wound well approximated in every point. Its whole track seemed to have become thickened, and was well consolidated. The catheter was replaced.

Before the operation the orifice of the urethra was very prominent. After the operation it was so much retracted that it was difficult to see it for the purpose of introducing the catheter. On the removal of the sutures it assumed its natural position.

The iron sutures were examined after their removal by means of a magnifying glass, and no crust of oxidation was visible upon them. They came away perfectly smooth, and left no ulcerated or irritated surface in their track.

6th. Up to the present time the bowels had not been disturbed, and for the purpose of dissolving the confined indurated feces, I ordered the rectum to be injected with one ounce of fresh ox-gall mixed with two or three ounces of warm water. This being the natural solvent of feculent matter, a very copious and soft discharge was effected in the course of two hours without straining, pain, or other difficulty.

7th. This morning I ordered the catheter to be removed three hours before the time for making my visit. During this three hours not a drop of urine escaped, and the bladder tolerated its presence without uneasiness. On introducing the catheter, four or five ounces of urine flowed out in a stream. The patient sat up to day for the first time.

8th. The same thing was repeated to-day. Before introducing the catheter I made a digital examination of the parts while the bladder was distended with urine, and all were apparently thickened and firm. The catheter was now ordered to be left out altogether, and to be used at intervals of two or three hours for drawing off the urine. The patient walked across the room to-day. Ordered another enema of fresh ox-gall.

9th. Allowed the patient to urinate without the catheter, but warned her against straining. She did not succeed in passing water, but felt satisfied

that she could have accomplished it by making a satisfactory effort. The injection yesterday had acted promptly, but was followed by tenesmus, which still continued. Ordered enemas of starch and laudanum.

10*th*. Yesterday afternoon and during last night the patient urinated without the catheter at intervals of two or three hours without the least difficulty, not a drop passing in any other way than through the natural outlet.

11*th*. Placed the patient for the last time on her knees before a good light, and in the presence of Drs. Clark, Atkinson, and Drysdale, inspected the parts. All present were delighted with the condition of things; there was a perfectly solid cicatrix, and so firmly closed from one extremity to the other as to defy all ordinary efforts to rupture it.

12*th*. To-day, just three weeks after the operation, my patient walked down stairs with very little assistance, got into her carriage and rode home, a distance of several miles.

Remarks.—There are several points in the above case worthy of remark. This patient had passed through eight parturitions without trouble, and yet connected with the ninth pregnancy a terrible accident occurred! Can this be explained by any peculiar condition, not existing in her previous labours, or are we to censure the medical attendants for improper or clumsy manipulations? As a prosecution was talked of against one of the medical gentlemen, it will not be out of place to briefly examine this question.

At 4 o'clock A. M., June 2, 1859, Dr. J. C. Weatherby was called upon. After waiting several hours—the pains being weak and inefficient, and the os tincæ being well dilated—at 9 o'clock A. M. he stimulated the uterine fibre with ergot, and excited active labour. At 10 o'clock A. M. he attempted to use the forceps, but failed in applying them, using no improper force. He then sought the assistance of Dr. C. F. Clark, who, by making some slight lateral motion of the blades, applied the forceps, and without much force the head was delivered at 2 o'clock P. M., the child being dead. Now here was active labour, in a sufficiently capacious pelvis, with a favourable position of the head, continuing only four or five hours! Every experienced accoucheur will at once conclude that the injury is too grave to be explained by so slight a cause. True, the uterus was acting, as is usual under the influence of ergot, without intermission, and the child may have fallen a victim to the delay in delivery after having established this condition of uterine contraction, yet it is scarcely conceivable that the total vitality of any portion of the female organs could be destroyed by such temporary pressure, and particularly when unaided by the employment of extraneous force. The forceps were at no time applied, excepting immediately before delivery, and as they never came in contact with the part injured, of course no injury can be attributed either to their application or their use. These inferences are corroborated by daily experience. Cases are frequently met with of very strong labour, lasting many hours, where

there exists a disproportion between the size of the head and the dimensions of the pelvis, and where delivery, even after ergot has been given and the forceps have been applied, is difficult and prolonged, yet no such serious consequences ensue as did in the above case. It is proper, therefore, to look for some pre-existing condition, rather than to parturition, to explain the cause of this fistula. What was that condition?

There was very great anterior obliquity of the uterus, so that its axis formed a right angle with the line of the body. This, no doubt, not only added greatly to the difficulties in the process of parturition, and in the application of the forceps, but, in my opinion, was the principal cause in the production of the fistula. The natural tendency of this position of the gravid uterus would be to elongate the vagina, and with it to drag up the bladder and curve it over the crest of the pubes. At the same time the vertex of the child's head would necessarily rest upon the crest of the pubes and be likewise firmly pressed against it by the posterior wall of the vagina and the corresponding portion of the cervix uteri being stretched tightly over the face of the child. The anterior wall of the vagina and the coats of the bladder would consequently be compressed between two hard bodies, and this compression being constant, and its force constantly increasing as pregnancy advanced, it may be readily understood how the vitality of the vesico-vaginal wall was destroyed, even before parturition commenced. Now, what are the facts as recorded by Dr. Weatherby? Two or three days before parturition set in, there was incontinence of urine. Before employing the forceps he tried to evacuate the bladder with the catheter, but found no urine. The patient had not urinated for two or three hours before Dr. Clark arrived, and yet on his arrival, both he and Dr. Weatherby introduced the catheter, and no urine was found. After the placenta was removed the catheter was again introduced, and with no better results. What became of the urine? It was not in the bladder, neither had the patient passed it in the natural way. At this very time, and for several days before, there must have been an opening between the bladder and the vagina, and it was this opening that gave egress to the urine, causing incontinence before parturition, and an empty bladder during and after it. But on June 3d and 4th the patient passed urine "all right," and no complaints were heard for three or four days after. This would seem to indicate that the vesico-vaginal walls were intact at this time. An important fact, however, is mentioned as having occurred at this period: there was great tumefaction of the labiæ. This tumefaction externally must have been dependent upon swelling within the pelvis. Now, every surgeon knows that, after the lateral operation for stone, the urine will flow through the cut for several hours; that then inflammation and consequently tumefaction will occur in the track of the wound, closing it up for the time, during which time the patient will pass urine "all right" through the urethra; and then, again, as the tumefaction subsides, that the urine will again escape

through the wound. This is precisely what occurred in the above unfortunate case: the fistula, having been formed before parturition, allowed the urine to escape, until the inflammation and tumefaction, following a difficult labour, closed up the opening, and caused the urine, for a time, to be passed per urethram; then, as the tumefaction subsided, the fistula opened, and the urine again escaped through it, causing incontinence.

This, I think, is the true and legitimate explanation of the cause of the vesico-vaginal fistula in the case above reported, and if so, it ought to be a perfect vindication of the skill and carefulness of the experienced and excellent accoucheurs who managed the case.

With respect to the surgical treatment of this loathsome affliction, there are several points to which I wish to call the special attention of the profession. To Dr. Sims, unquestionably, belong the credit and lasting honour of having originated the proper management of these cases, and his mode of operating will, no doubt, be the main guide to surgeons in the future. Yet, now and then, little improvements will suggest themselves to every operator, which may greatly contribute to the facility and success of the operation in the hands of less dextrous surgeons, and thus, instead of diminishing the high reputation of the discoverer, will add another stone to his enduring monument. It is with this view alone that I have been induced to offer this case for publication. The points referred to are:—

1. *Tangling of the Long Ends of the Sutures.*—Every operator for vesico-vaginal fistula is more or less annoyed by the tangling of the numerous long wires protruding from the vagina, after having carefully inserted all his sutures. He undertakes to adjust the edges of the fistula, and to fasten each wire to its fellow, or to pass the two corresponding ends through the hole in the button, and he finds them crossing each other in every way. In order to overcome this difficulty he must first expose the fistula to view, pass in a blunt hook, catch the wire desired, rake it out and hold it until he does the same with its fellow. After doing this, he then adjusts the edges of the wound and secures the suture. This same process is repeated with every suture, which makes this part of the operation troublesome and tedious. With the simple arrangement of the thread, however, as shown in Fig. 1, all this difficulty is obviated. The corresponding ends of each suture can be at once selected without exposing the fistula and without losing time in raking out the wires, while at the same time this plan will prevent all possibility of mistakes in bringing different wires together.

2. *The Coaptation of the Edges of the Fistula and the Fastening of the Sutures before the Application of the Plate.*—By means of Dr. Bozeman's suture adjuster, the raw edges of the fistula can be readily drawn together; but by the time this has been done through a series of sutures, it will be found that the constant traction on the weak wires by the muscular walls of the fistula, will have more or less deranged the parallelism of the suture wires, and consequently the accurate coaptation of the edges of the

wound. The whole support of the wound must in this case depend solely upon the *stiffness* of a wire, which has been intentionally made as soft and pliable as art can make it, so that it may, in this respect, assimilate the character of a thread. The button now being slid over these *loose* and easily deranged wires, the fistula is hid by it from the eye of the surgeon, and the operation is completed without a positive certainty that the adjustment of the edges of the wound is perfect. This, I think, is a great defect in Dr. Bozeman's plan of operating. But the twister, Fig. 2, introduced by Prof. Simpson, is as perfect a suture adjuster as that of Dr. Bozeman, and has the additional invaluable advantage of enabling us to secure the sutures permanently at the very moment when the edges of the fistula are satisfactorily in apposition. This little instrument will, likewise, of itself accomplish this object much more rapidly, neatly, and securely than the fulcrum and forceps of Dr. Sims. The point, however, to which I desire to call special attention, is the perfect and unalterable coaptation of the raw edges of the fistula previous to the application of the button, and which is done, with the greatest certainty, by my method of securing by the twister every alternate suture.

3. *The Button or Plate.*—The button, which is intended to act both as a splint and a cover to the fistulous opening, originated with Dr. Bozeman. It has been modified by Dr. D. H. Agnew, and improved by Prof. Simpson. I have used the Bozeman button, but never to my entire satisfaction, because, while it answers as a splint in the longitudinal direction, it entirely fails in supporting the sides of the fistula—an object of the greatest importance. Neither can it be considered as a cover to the wound, offering complete protection, at all times, from injurious discharges. At first, its adjustment may be accurate and firm, but in a few days I have found it becoming loose, and by the time the sutures were to be cut, and even before, the button was tilting and wabbling—certainly not answering the purpose of a lateral splint or a cover. The button, modified by Dr. Agnew, it appears to me, must act precisely upon the same principle. Prof. Simpson's iron-wire splint, while not intended as a cover to the parts, meets the object desired by supporting the wound both laterally and longitudinally; but I should think that, without great care, there might be some danger that, instead of bringing the edges of the fistula in close apposition, it would have the tendency to keep them apart. This danger would certainly be greatly increased in certain fistulæ where a deep hold could not be taken with the sutures. Besides, its unyielding nature—being made firm by twisting several strands of wire together—would most likely cause it to irritate the more yielding soft parts bearing upon its two ends. The plate, however, constructed by myself, Fig. 4, possesses the advantages of both Dr. Bozeman's button and Prof. Simpson's splint, without having any of the above objections. By means of the fenestrum, through which I pass the previously twisted wires, the sutures can be secured by the perforated

shot, as in the Bozeman button, and by means of the parallel holes on each side, through which I pass the remaining wires, the sutures can be secured by the twister, as in the Simpson splint. In addition, there is this very great advantage over both, that before the application of my plate the edges of the fistula have already been accurately and immovably adjusted. Besides, as occurred in the case reported above, the fenestrum itself acts the part of a suture in maintaining the edges in contact.

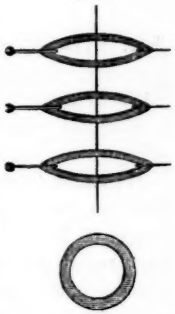
In comparing the length of the fistula, Fig. 3, which is half size, with the length of the plate, Fig. 4, full size, a disparity will be noticed. It is not necessary to have the plate as long as the original wound. The fistulous opening has been expanded by muscular retraction, and after the edges have been adjusted, its length may be diminished by affixing a shorter plate, by which means the cicatrix will be shorter, thicker, and firmer, than to leave it without a splint, as Dr. Sim's does, or to heal it under a button as long as itself.

4. *Metallic Sutures.*—While Dr. Sims asserts it as a truth, "that the great success of these operations is due entirely to the silver wire," Prof. Simpson believes that blue iron-wire is the best. Of the two metals, blue iron is certainly the cheapest and strongest, and as it is not readily oxidizable, and is highly malleable and ductile, it possesses in a high degree every property belonging to silver, while in other respects it is preferable. In the case above reported nothing could have answered the purpose better. The sutures came away easily and without tarnish, leaving no ulcerated or irritated track. With regard to the use of iron sutures, and the effect of human tissues upon them, I can speak from a very long and large experience. Besides employing the blue needle as a frequent suture in ordinary operations, I have used them over fifty times in the operation of gastrotomy. In every instance, save one, I have transfixed the wound through the walls of the abdomen by means of the blue toilet needle, and it has never been the cause of irritation to the soft parts, nor has it been oxidized by the animal tissues. The only cause of irritation was the thread employed to complete the twisted suture, and which I have recently abandoned for a more suitable material. In the case, in which the iron needle was not used, I employed the silver sutures, and this was done on the strength of the recommendation of Dr. Sims, in his Anniversary Discourse on "Silver Sutures in Surgery." The ovarian tumour removed in this instance weighed sixty-three pounds, and consequently the parietes of the abdomen were greatly relaxed, while the viscera were very much contracted, giving to the wound no under support. It was impossible, under these circumstances, to make a neat dressing with the silver wire, and the incurvation of the edges of the wound was a source of constant annoyance, even long after the patient had recovered and was walking about. I am satisfied that this suture, or any other soft wire suture, is wholly unadapted to wounds of this cha-

racter, as it cannot support the relaxed borders of the wound and maintain the edges in accurate apposition.

Since performing the above operation for vesico-vaginal fistula I have originated a new suture, which, I think, may displace all other forms of metallic sutures. It consists essentially of the blue steel toilet needle and a small gum elastic ring. The needle first transfixes the wound, and then the ring is looped over both ends. It makes an exceedingly neat dressing, and is rapidly applied, by first looping the point of the needle and then stretching the ring over the head, when its own elasticity at once adjusts it. It is perfectly clean, and, like metal, absorbs no moisture, affording no lodgment to putrefying and irritating discharges. It expands and contracts as the varying condition of the wound requires, always keeping the edges in apposition, while, at the same time, it does not ulcerate through the skin, as does the unyielding and absorbing material of the ordinary twisted suture. The ring may be made of any size by cutting off smooth sections from gum-elastic tubes of different calibres. Those I have used were procured at Thornley's, No. 311 Chestnut Street, and were made for a different purpose. I employed this suture for the first time on the 6th of October, after an operation for the removal of an ovarian tumour, and within two weeks afterwards in three additional cases of the same kind. It answered a most admirable purpose, and every gentleman who witnessed its application expressed his delight. Fig. 8 exhibits the size of the ring and the mode of its employment in the new suture. By inspecting this figure, it must be apparent that nothing would simplify the operation of vesico-vaginal fistula more, and, at the same time, secure the edges of the wound better than this suture. The lateral support of the wound would be perfect, the wound could not become incurvated, and the mucous coat of the bladder could not protrude. The needle points could be readily guarded by perforated shot clamped upon them, and removed, when desired, by wire nippers.

Fig. 8.



5. *Fresh Ox-Gall Enemata.*—The long confinement of the bowels after these operations leads to more or less accumulation of indurated feces in the rectum, while the result of such a condition of things will manifestly be an undue effort in defecation, unless the hardened mass be softened. The best solvent of the indurated contents of the bowels, according to my experience, is the bile or gall of the ox, and this injected into the rectum will cause an easy dejection, free from that straining, which might hazard the integrity of the tender cicatrix. The fresh gall-bladder can at all times be procured at the butcher's shambles, and this will contain an ample supply for our purpose.

6. *Incrustations on and in the Catheter.*—In the above case it was exceedingly troublesome to keep the catheter clean. Although I attended to it myself twice a day, washing it thoroughly with hot water and scrubbing its interior with a rod of whalebone, yet the deposit was so rapid that by the time I dispensed with the use of the catheter, it was nearly obstructed by a solid lining of phosphatic salts. The incrustation was very difficult to remove, and pieces that were scaled off were as hard as a urinary calculus. In order to arrive at a good and suitable solvent, small portions of the deposit were subjected for twenty-four hours to the action of different agents. A solution of acetate of lead (which has been recommended to be injected into the bladder as a solvent in phosphatic calculi), ammonia, glycerine, and alcohol, had no action upon it. A solution of potassa, chloride of zinc, and strong sulphuric acid softened it. Muriatic acid nearly dissolved it, and nitric acid made a perfect solution. Hence the best liquid for keeping the catheter clean, where this incrustation proved troublesome, would be water acidulated with nitric acid.

ART. VII.—*Aneurism of the Aorta pressing upon the Trachea and causing violent paroxysms of Asthma, with statistics of 22 cases of Aneurism of the Thoracic Aorta, from the records of the New York Hospital.* Reported by BARTOW DARRACH, M. D., of Quincy, Ill.

WILLIAM CATLIN, aged 44, native of Maryland, seaman. Admitted into the New York Hospital, August 13th, 1858, and placed under the charge of Dr. H. D. Bulkley. The patient says he has had a slight cold for a month past, with some mucous expectoration. Four days ago, while coming down the North River, as a hand on one of the barges, he was suddenly seized with a severe paroxysm of asthma. He never had such an attack before, and can assign no cause for it. He expressly says he had not strained himself, nor can anything indicating the formation of aneurism be discovered in his history. He knows of no tendency to phthisis, rheumatism, or gout.

Since the one mentioned above, he has had a distinct paroxysm each day. This morning he started for the Brooklyn Hospital, but had another attack on the way, and was picked up and brought here by the police.

When admitted he was suffering intense dyspnœa—face livid and anxious; surface cool and perspiring; pulse 132, full and strong; expansion of the chest, notwithstanding great effort, very little; inspiration and expiration, especially the latter, wheezing and stridulous. His chest is resonant throughout, large and well formed. On auscultation a loud sonorous rale obscures all other respiratory sounds. Heart sounds natural,

and voice unaffected. Hoffmann's anodyne, chloroform, expectorants, lobelia, belladonna, and various other things were tried, in succession, without avail. His paroxysms would last from three to six hours. During their height he often became delirious.

From the 18th Aug. to Sept. 20, his paroxysms were much less frequent, and at very irregular intervals. His general condition improved, but more or less wheezing expiration and bronchitis persistently continued. During this period repeated and very careful examinations of his chest only elicited a very slightly appreciable dulness below the right clavicle, with a loud tubal sound, most distinct here, but heard more or less over the whole chest. Nothing like a bruit could ever be heard.

Early on the morning of the 20th he died suddenly, in a paroxysm.

On *post-mortem* examination, an aneurism about the size of a walnut, with an opening a little larger than a ten-cent piece, was found arising from the posterior wall of the arch of the aorta, which pressed directly upon the bifurcation of the trachea. It remained free from attachment to any of the adjacent organs. It was filled with soft coagula, and at its bottom was a plate of hard osseous deposit. Other spots of atheroma were found scattered through the aorta. No other lesions were found after an examination of the whole body.

Statistics.—On looking over the case books of the hospital, I find the notes of twenty-three cases of aneurism of the thoracic aorta, including the one just given.

The average age of 22 was 45 years, minimum 20, and maximum 70 years. Excluding four cases, in which the aneurism was very manifestly consequent upon cardiac hypertrophy, and in which the symptoms were lost in those of the disease of the heart, the average age is reduced to 34¼ years.

Fourteen were seamen; of boatman, steward of a steamer, coachman, carriage trimmer, butcher, gardener, bar-keeper, and keeper of a street stand, each one. No cause is mentioned, excepting in two cases. One had lifted a heavy weight a few weeks before, and the second felt something give way in his chest, and a warm gush, immediately after sexual connection.

Of the twenty-three cases, the aneurism occurred in the ascending aorta in three. Pain in the epigastrium was the only symptom noted in one of them, which ruptured into the pericardium. In this case a true aneurism arose from the junction of the heart with the aorta. In the other two cases, the symptoms of a false aneurism were merged in those of pleurisy in the one case, and of old cardiac disease in the other, of which the patients died.

Eight of the aneurisms arose from the arch. Three of them, the 1st, 15th, and 21st, were false, and connected with valvular disease of the heart in the 1st, and with fatty degeneration in the other two. These

latter fell in the street, and were brought in moribund. The 1st had had pain in the breast and bronchitis for eight months, and died from rupture into the posterior mediastinum. In the 3d, 4th, 17th, and 23d the aneurism pressed upon the trachea, causing severe dyspnœa; in three of the instances paroxysmal. All excepting the 17th were characterized by a respiration which is described as crowing, stridulous, or wheezing. Dysphagia was a marked symptom in all but the last mentioned case. The third suffered from a violent pain in the side of the neck, and œdema of the face. His earliest symptom was dyspnœa. In this case a pulsating tumour appeared externally at the right sterno-clavicular articulation, and attained the size of a fœtal head. The 4th had been very much prostrated by a remittent fever, and a vigorous antiphlogistic treatment before admission. The aneurism was complicated with hypertrophy of the heart, and incipient phthisis. The 17th was taken suddenly, while aloft, with vertigo and dyspnœa. He had aphonia and bronchitis. All of those arising from the arch, excepting the 1st and 23d, whose history is given above, died apparently of exhaustion. The 6th died with pneumonia. The aneurism was only discovered after death. He had chalky concretions in his lungs.

In five cases the descending aorta was the seat of the lesion. Three of these were false. Two were associated with disease of the heart. All caused death by rupture, and in all there was caries of the vertebra. None produced any characteristic symptoms of their presence. Two, the 2d and 20th, complained of lumbago; the 13th of pain in the side. In the 12th the aneurism pressed upon the left bronchus, causing wheezing respiration, and feebleness of voice. The second ruptured into the spinal canal.

Six cases left the hospital. In another no *post-mortem* examination was allowed. This latter has been reported by Dr. Cameron, in the *New York Medical Press*, Sept. 1859. It, with two others, was distinguished by a peculiar loud ringing cough. Three presented a pulsating tumour externally, in two instances with a bruit; in another with downward displacement of the heart. One case was diagnosed by the bruit alone. The only other symptom was pain in the epigastrium and breast.

In the majority of cases pain seems to have been the earliest manifestation, as well as the most frequent and most constant in the course of the disease. It was the first symptom in thirteen out of seventeen cases, where the first symptom is mentioned, and existed in five-sixths of all the cases. It was sudden in its onset in three. No diagnostic value could be attached to its locality. Thus, of thirteen cases, it was in the left side of the neck, in one case; between the shoulders, and in the breast, one; nucha, one; epigastrium, right arm, side of the neck, and shoulder, and breast, each one; in the left side in three; and in the loins in two. The next symptom in frequency is cough, which occurred in ten cases, and is described as crowing, stridulous, or ringing; and laryngeal in three in-

stances; spasmodic in one. The other symptoms are noted as follows: Bronchitis in 5; emaciation 5; bruit 4; external tumour 4; dysphagia 3; dyspnœa 6; wheezing respiration 3; palpitation 3; tracheal rales 3; œdema and lividity of the face 1; displacement of the heart, tenderness of the tumour, convulsions, aphonia, and anæmia, each 1. After the bruit and the appearance of a tumour externally, the symptoms of interference with the respiratory organs seem to be the most uniformly characteristic. The wheezing respiration especially, was uniformly associated with pressure upon the trachea, or one of the large bronchi. In one case it was the diagnostic symptom. But in the aneurisms of the descending aorta, these symptoms were wanting, excepting in one case. A bruit is mentioned in but four cases, although it was carefully looked for in others.

The termination by rupture seemed to be most uniform in the descending aorta, while those in the arch generally terminated by exhaustion, or some accompanying disease.

QUINCY, ILL., Oct. 15th, 1859.

ART. VIII.—*Case of Compound Comminuted Fracture of the Patella.* By J. LEVERGOOD, M. D., Lancaster, Pa.

ON the nineteenth day of February, 1859, a young man, named Patrick McKeefer, aged about thirty-five years, was admitted into the Lancaster County Hospital under the following circumstances:—

About eight or ten weeks previous to his admission, while engaged in driving a stage, he was kicked by one of his horses on the left patella, producing a compound comminuted fracture of that bone. At the time of the accident, and for some time subsequently, the hemorrhage from the wound was profuse. He was brought to this city and placed in charge of a physician, but after being under treatment about four weeks, he very imprudently left his bed without the knowledge or consent of his professional attendant, and attempted, with the assistance of a crutch, to walk upon his limb. The result of his temerity, of course, was disastrous; the imperfectly united fragments were again widely separated, and the joint became stiff, swollen, and excessively painful. He was again placed under treatment, but circumstances occurred rendering his removal to the hospital necessary.

On his admission I found the knee stiff, very slightly flexed, exceedingly sensitive upon the least motion, and so much distended by an accumulation of fluid as to make it almost impossible to distinguish the number and situation of the fractured portions of the rotula. The slightest palpation of the tumefied mass afforded to the hand, very perceptibly, the sense of fluctuation, and the most prominent part of the swelling was on the anterior part of the joint. His general health was excellent.

Although almost every expedient had been exhausted, previously to his entrance into the hospital, with the view of securing the absorption of the fluid contained in the joint, and a reunion of the fractured bone, it was deemed advisable, after consultation with my colleagues, to make another attempt to preserve the patient's limb. I therefore gave him alterative doses of calomel and opium, applied tincture of iodine, and carefully bandaged the leg and laid it in a splint. This course of treatment was sedulously persevered in for about one week, but there appearing to be no visible improvement, and as the fluid was beginning to burrow under the extensor muscles of the thigh, I at once made an incision into the joint and evacuated about twelve ounces of pus.

Feb. 28th. Has not rested well; no appetite; and the accumulation of fluid is nearly as great as before the joint had been opened. Made another incision on the side opposite to the first one, and removed ten ounces of purulent matter. As the matter had invaded the interstices of the muscles of the thigh, there was considerable handling and compression necessary in order to dislodge it from the sinuses in which it was deposited. Ordered tonics, generous diet, and porter daily.

March 4th. A profuse sanious and unhealthy discharge going on from both incisions; and the synovial fluid is, and has been for some time, escaping. Notwithstanding, the patient's health remains unimpaired, his appetite is good, and his sleep is tolerably refreshing. At this stage of the case I suggested the propriety of amputating the leg, keeping in mind the great length of time since the injury was received—the exhausting effect of the profuse and unhealthy discharge then going on—the many futile efforts, thus far, made to restore the limb to usefulness—and recollecting that South, in his translation of Chelius, tells us that "compound fracture of the knee-cap *almost invariably* requires amputation, as the injury producing it is so severe that there can be little expectation of a satisfactory issue," I considered this a case requiring and justifying the use of the knife. But the patient not being willing to submit to this supposed *dernier ressort*, I continued the treatment as heretofore, and at the expiration of my term of hospital duty (first of May), I had the unfeigned pleasure of seeing my patient discharged well, with no other inconvenience than that of a slightly anchylosed knee. He walks without a staff, is engaged again in driving stage, and says his leg is "nearly as good as ever."

The above is a condensed account of this case as detailed in a paper read before the Lancaster City Medical Society, and it furnishes us with another proof of the propriety, when there is a preternatural accumulation of fluid in the knee-joint, of making an incision and evacuating it. In the August number of the *Lancet*, there are recorded three or four cases illustrating the feasibility of this procedure, and some of the cases, in many respects, are very analogous to the one above related.

ART. IX.—*Case of Epilepsy cured by the Removal of a Portion of Depressed Bone from the Skull, resulting from an Injury received ten years prior to the Operation.* By H. P. YEATES, M. D., of Baltimore. (With a wood-cut.)

ON Friday, 21st October, I was called to a young man, named George Eckhart, residing on Low St., æt. 20 years, who had been subject to epilepsy for ten years. He was now labouring under convulsions, and had been so for several hours previous; bowels torpid; without fever; ordered cathartic of prot. chlor. hyd. et ex. colocynth comp.

Saturday, 22d. No convulsions; cheerful.

Sunday, 23d. Eleven convulsions within an hour, and my attention was then called to the fact that ten years prior to this time he had received an injury on the head by a blow from a pick, such as used by labourers for excavating. Upon examination of the skull, there was found to be a considerable depression of a portion of the right parietal bone. Inasmuch as the first convulsion had followed the blow, and continued ever since, the longest interval being a month, and latterly but a few days, I determined to apply the trephine, and remove the depressed portion of bone, which I proceeded to do, assisted by Drs. E. S. Baldwin and Mahan. At this time (3 o'clock P. M.) the patient was insensible. I removed two buttons of bone, and the piece dividing the openings, being full two inches in length and three-quarters of an inch in breadth. The pulse, which before the operation was small, now became fuller, and the laborious respiration which had also existed gradually subsided; his comatose condition remained, and he had seven covulsions during the afternoon and night.

Monday, 24th. Pulse, 160; tongue, brown and dry; skin, hot and dry; coma continues; dilatation of the pupils; respiration hurried, without stertor, which existed on the previous day; bowels constipated. ℞.—Prot. chlor. hyd. x grs.

Tuesday, 25th. Pulse 160; tongue and skin as on the day before; pupils continue dilated; four convulsions since last visit; bowels not moved. Ordered enema of spts. terebinth. ℥j, ol. ricini ℥j, in a pint of warm water.

Wednesday, 26th. Pulse, 140; tongue moist; skin dry, but not so hot as before; pupils natural; no convulsions; bowels moved freely during the night, and partially conscious, with much less coma. Ordered 10 grs. chlor. pot. in water every three hours.

Thursday, 27th. Pulse, 100; skin and tongue moist; pupils natural; no convulsion; bowels moved; sensibility and consciousness perfectly restored, and complains of soreness of the head; wound healing.

Friday, 28th. Pulse, 100; tongue moist; pupils natural; no convulsion; bowels moved; perfectly sensible. ℞. continued.

Saturday, 29th. Pulse, 97; tongue moist and clean; no fever or convulsion, and perfectly rational. R. continued.

Sunday, 30th. Pulse, 80; no fever; all the functions natural. Suspend medicine, and dress the wound with simple cerate and lint. Diet, toast and tea; small piece of mutton-chop for dinner.

From this time the patient continued to improve, and now, four weeks since the operation, has had no convulsions, except those mentioned as occurring the second day after, and the wound in the scalp, which was of considerable size, has very nearly closed.

The most remarkable feature in this case is that on one of the buttons of bone removed is a tooth-like process (see fig.) proceeding from and perfectly attached to the piece, about one inch in length, which had no doubt been pressing on the brain, or projecting into the substance since the occurrence of the accident, and which I believe had been the cause of the convulsions.

BALT., Nov., 1859.

ART. X.—*Amputation of a Gangrenous Arm.* By B. J. D. IRWIN, M. D., Assistant Surgeon U. S. A.

February 3, 1859. A robust muscular Mexican, æt. 28, while handling his musketoon accidentally discharged it; the piece was charged with ball cartridge, and had the wooden ramrod in the barrel at the time. The contents of the gun entered the right arm on the anterior ulnar side about two inches above the wrist-joint, and thence passed obliquely upwards and outwards to a point about three inches above the elbow-joint, which it shattered as well as the lower end of the humerus. The ball passed out above the joint, while the ramrod broke and splintered into several parts, some of which made their exeunt opposite to the articulation. Many pieces of the rod were removed before I saw him. I examined the wound four hours after the receipt of the injury, and cleared it of all extraneous substances that could be detected. A long ball-forceps was passed through the course of the ball, and every particle of foreign matter removed *that could be found.* After satisfying myself of the extent of the injury, I warned the patient of its serious character, and the probability that amputation would be necessary in consequence of the dangerous nature of the lesion. He, being a poor man, depending on his manual labour for support, was anxious to retain the arm at any risk; I determined to use every means in my power to save the limb, even in a damaged condition. Cold water dressing ordered to be continually applied while the limb was placed in the

proper condition for its recovery in a semi-flexed position. Morph. sulph. gr. ss given, which tranquillized the patient.

4*th*. Slept well; limb very much swollen, painful. Continue cold application; morph. sulph. gr. ss, at bedtime; diet to be of the mildest character.

5*th*. Rested well; pulse good; extensive tumefaction all over the limb; slight crepitus on the dorsal aspect of the hand; profuse secretion of fetid gas in the wound; wound carefully cleansed, and washed with dilute sol. sodæ chlor.; cataplas. carb. lig. substituted for the cold water dressing. Ol. ricini f℥iss, to be taken at bedtime.

6*th*. Suppuration very profuse and excessively fetid; no abatement of swelling; bowels freely moved; slept well; tongue natural; appetite very good. Continue previous treatment; and allowed light nourishing diet.

7*th*. No diminution of the suppurative process; extensive generation of fetid gas throughout the arm; discoloration and loss of sensation in the hand; *limb gangrenous*. Continue cataplasm of pulv. carb. lig., and frequent ablution with "Labarraque's solution." Patient rests well, and is of a resigned but cheerful disposition; appetite good; bowels regular; tongue and pulse natural.

8*th*. Gangrene extending upwards; discharge excessively offensive; phlyctænæ numerous over the arm; total loss of sensibility in the limb below the elbow-joint; patient is weak, and inclined to be restless; is now anxious to have the limb removed; pulse quick and small; tongue clean, bowels natural. Continue treatment of preceding day.

8 o'clock P. M. I was summoned in haste to visit this patient, who was reported to be "bleeding to death" from his wounds. I arrived in a few moments and found he had had a profuse hemorrhage from the lower wound by which he had lost about three pints of blood. Previous to my arrival a handkerchief had been tightly tied around the upper portion of the arm, which had controlled the hemorrhage already; but, as he complained much of the pain which it caused him, I loosened it and placed a tourniquet with a proper pad over the vessel. The patient was alone and asleep when the bleeding commenced, hence the amount of blood lost before he received assistance. He is very weak, cold, and complains much of the pain caused by the tourniquet; pulse 70, weak; countenance pallid. ℞.—Ammon. carb. ℥j; æther sulph. ℥iv; tinct. zingib. ℥ij; spt. vin. gal. f℥iij.—M. A dessertspoonful in a little water every half hour when not sleeping. Morph. sulph. gr. ss. This gradually restored his strength, and about one hour after the cessation of hemorrhage, the pressure of the tourniquet was gradually lessened, and a careful assistant left to take charge of the patient with proper directions how to proceed in case of a recurrence of the bleeding. I visited him during the night and found him much revived, although sleepless, in consequence of which I repeated the morphia.

9*th*. Slept well during the latter part of the night; feels much stronger,

and is desirous to have the arm amputated. Pulse good; is cheerful and easy. At 9 A. M., I had him removed from his tent to the hospital, when, all things being in readiness, chloroform and sulphuric ether, mixed in equal measures, were cautiously administered until he was brought under their influence. Having done this, I directed an assistant to make pressure upon the axillary artery with his fingers, and I then removed the limb by an internal and external flap operation, about five inches above the gangrenous inflammation. But a few ounces of blood were lost, and the wound being united by interrupted sutures and adhesive straps, cold water dressing was applied to the stump. The anæsthesia passed quickly off, and the patient did not complain of any pain whatever. Diffusible stimulants and an anodyne were given, after which he enjoyed refreshing repose.

The removed limb was found gangrenous up to the elbow-joint, the course of the ball being in a state of sphacelus; the lower end of the humerus shattered to pieces with both condyles entirely separated from it. The elbow-joint was full of pus and undergoing rapid disorganization; the artery had ruptured opposite the joint, about half an inch above its bifurcation. Two pieces of the ramrod were found deeply imbedded in the soft tissues on the *dorsal aspect* of the arm, one three and the other six inches in length. These pieces of wood had come in contact with the bone and rebounded backwards, making a new track for themselves entirely distinct from the course of the ball. It was impossible to discover them as they lay under the deep layer of muscles, which difficulty was increased by the tumefaction.

10*th*. Patient spent a comfortable night; removed dressing and found the wound looking well—no appearance of gangrene. Continue cold application; generous diet; ol. ricini f℥iss. Morph. sulph. gr. ss at bedtime.

11*th*. Continues doing well; wound suppurating freely; no union by "first intention;" pil. quin. sulph., three times daily. Continue other treatment.

14*th*. Convalescing rapidly; wound healing by granulation; suppuration free and of healthy character; two small sloughs forming on the edge of the external flap.

16*th*. Doing well. Removed sloughs from wound; three ligatures came away; general health good.

This man continued to improve rapidly without a bad symptom. On the sixteenth day after the operation the last ligature, from the brachial artery, came away, and twenty-seven days after the amputation he was discharged in excellent health and spirits.

This case presents some interesting points in its history: First, the nature of the missiles inflicting the lesion, and the unusual manner by which the foreign body became hidden in the soft tissues. Second, the gangrenous condition of the limb at the time when amputation was resorted

to, and the cessation of the gangrene with the removal of the gangrenous part, before a line of demarcation had been established.

I am well aware of the hazardous nature of operating *during the progressive stage of gangrene;* but with the rupture of the chief arterial vessel, I had to amputate or have recourse to ligation of the artery near the trunk, which would have been courting the extension of the gangrene in as dangerous a degree as by resorting to amputation at once. Pressure would not do; it could not be borne, and the short period during which it had to be resorted to it caused a very marked rapid extension of the gangrene towards the shoulder. Under these circumstances I was induced to anticipate the disease by removing the offending member at once, knowing that, in case the gangrene should reappear, I could amputate at the shoulder-joint.

FORT BUCHANAN, ARIZONA, June, 1859.

ART. XI.—*Retention of Urine from Chronic Enlargement of the Prostate Gland; Supra-pubic Puncture of the Bladder after seventy-two hours; Restoration of the Natural Passage; Death after six weeks from Inanition.* By B. F. SCHNECK, M. D., Lebanon, Pa.

August, 1858. Thomas Light, aged 58, of lymphatic temperament, had been suffering from urinary trouble for the last ten years. He never consulted a physician; each attack ending favourably after several days' acute pain. About the 22d of August, however, after exposure, he experienced severer symptoms than ever before; and having on the 24th sent for some medicines, which did him no good, he on the morning of the 25th requested my attendance. He stated that he had last made water on the evening of the 23d, thirty-six hours before; and then only a small quantity and with great agony. The bladder was greatly distended and tender; and he suffered severely from constant inclination to urinate, but could not pass a drop. I made many and patient attempts with the catheter; using, in succession, the silver, gum, and flexible metal instrument, of all sizes, without being able to pass the prostate. Every attempt to introduce it further occasioned the greatest agony. Ordered an active saline cathartic and warm baths, and tried the catheter again at noon and evening, but with the same want of success; while on each occasion, then and subsequently, the external end of the instrument was observed to pass through various irregular motions, indicating deviations of the urethra from its natural, median course. This was confirmed by a finger in the rectum, which revealed the point of the catheter quite turned over against the ramus of the left ischium. Evening.

The purgative in the morning having been rejected, and there being now much fever, he was bled to twenty ounces, and was ordered to have several comp. cath. pills and Epsom salts; warm bath to be continued.

26*th*. Has passed no water, and is not purged; but vomited repeatedly during the night. His expression is very anxious, and he sleeps a great deal (has had no anodyne), indicating evidently urea poisoning. Drs. Marshall and Rauch in consultation; no better success with catheter in afternoon and evening. Bowels moved three times since noon. Evening. Pulse 72, full and regular; very drowsy; abdominal distension enormous. In this dilemma, the man's only hope seemed to lie in puncture of the bladder; and after a final consultation, in which the necessity of the operation was unhesitatingly admitted (after another futile essay with the catheter all round), it was accordingly done, an inch and a half above the pubis. The common straight ascites trocar, with canula two and a half inches long, in the absence of any other, was used. Fully three pints of dark-coloured, most offensive urine flowed through the tube; after which a gum catheter was introduced through it, and both having been secured by tapes and adhesive plaster, the patient was put to bed tolerably comfortable.

27*th*. Morning. Has had a good night. At least two pints of urine was collected through the catheter, and probably four ounces more came through the canula, and was absorbed by cloths. Very comatose, pulse slow and feeble, and skin cool; face pale and very anxious. Adynamia, from urea poisoning. Ordered a dessertspoonful of brandy every hour, until reaction had set in. At noon and evening the patient felt better; had taken meat broths with relish; was warmer and less drowsy.

28*th*. Morning. Scarcely any urine passed during the night; abdomen much tumefied, presenting exactly the outline of the distended bladder before the operation; great pain over the whole region, and did not sleep a wink all night. Placed him on his side, when the urine again flowed. Pulse 68, calor mordax, has great thirst; complains of a painful stricture in his throat, and says he can scarcely swallow. Ordered opium gr. j, sp. nitric ether f℥j, diluted largely, to be taken every fourth hour; omitting brandy. Evening. Symptoms improved; attempted catheter for first time since operation, without success.

29*th*. Strong tendency to sinking again, with coma, &c. Resumed brandy and vol. julep.

30*th*, 31*st*. Better; tried catheter again, including Dr. Physick's bougie-tipped instrument, but unsuccessfully. It was noticed, however, that the urine that flowed from the puncture immediately after, for the space of a few minutes, was deeply tinged with blood; a most encouraging sign, as indicating a communication between the urethra and bladder. Noon and evening. General condition could not be better; may be pronounced well as soon as the urethra is again open.

September 1. Glans penis and orifice of urethra moist, as with several

drops of urine. Catheter attempted again by several medical friends, but without success.

2*d*, 4*th*. Could not be better. Has an excellent appetite, and seems to have taken a new lease of life. My friends again attempted the catheter, but unavailingly.

5*th*. Tongue dryish, and of a bright-red colour in the middle; pulse 84.

6*th*. Tongue very red, and covered with a tenacious, pasty mucus, as in enteric fever; an unfavourable sign.

7*th*. At the first attempt this morning, without any pain, the catheter entered the bladder, and was left in sitû. Now also a new gum catheter having been introduced through the canula, this latter was withdrawn, so as to allow the tissues to contract upon its course, preparatory to the final removal also of the gum tube.

8*th*. Diarrhœa with tenesmus; for which laudanum enemata, and catechu and creta mixture were used. This rectal irritation may be owing to the presence of the catheter, and may require the removal of the instrument very soon. Tongue dry and red; delirious occasionally.

10*th*. The catheter having slipped almost entirely out of the urethra, I removed and reintroduced it, and drew off a large quantity of urine, very acid, and containing much purulent matter. Has had several evacuations resembling soapsuds. Pulse 92, tongue red and dry, face anxious. Gave volatile julep, and infusion of serpentaria and valerian. Evening. In proceeding to introduce the catheter, the orifice of the urethra was observed to be closed by a thin incrustation of blood, which had formed since the morning. As soon as this film was removed, about ʒss of blood and pus mixed ran from the urethra. The instrument gave pain about two inches down, and again in passing the prostate. The first urine that flowed was one half blood; then it became clear, and lastly was so heavily mingled with pus as to have quite a milky colour. Decided to lay the catheter aside, and introduce it only twice a day. Orifice of puncture almost closed.

12*th*. Since the removal of the catheter, there have been no more tenesmus and diarrhœa. Prostration very great; extremities cold; pulse 78 and thready; bowels moved several times, evacuations consisting of fecal matter and blood. Ordered brandy, quinia, and beef essence. Evening. About the same; a little urine passed through the natural passages, by the patient's own volition.

13*th*. Is better; pulse 89; surface warm; tongue moist; bowels quiet. A little urine flowed through the puncture; and he several times relieved his bladder in the natural way, and freely.

14*th*, 16*th*. Pulse 74; tongue moist; evacuations perfectly natural; apparently almost convalescent.

17*th*. Tongue dryish; pulse 96; Huxham's tr. bark.

18*th*. Pulse 100; skin shrunken, cold, and dry. Vomited the bark tinct.

19*th*. Passed urine in a full, thick stream during the night. Pulse 84 and feeble; vol. julep.

20*th*, 21*st*. Passes his urine by the urethra at will, except a few drops, which will ooze from the puncture. Brown incrustations have appeared on his teeth; pulse varying from 90—98.

22*d*—25*th*. In statû quò. Pulse 80—86; takes brandy, valerian, eats animal and vegetable food with great relish, and seems to be gaining flesh. Bowels natural.

28*th*—30*th*. The urine again containing pus, uva ursi was given; the other remedies being continued. Pulse 86—96; tongue dryish and red.

October 2. Morning. Pulse 120; tongue red and dry; countenance bad; sinking rapidly. Evening. Pulse 140, and very feeble. It is very remarkable that eight days ago, and two weeks ago, the symptoms underwent a precisely similar change for the worse, notwithstanding the use, at intervals, of quinia and tinct. bark, which, it might be supposed, ought to have been capable of setting aside any *periodical, pernicious* movement in the case.

3*d*. Pulse 130—140; tongue dry and red; urine quite clear. Complained again of pain and difficulty in swallowing; says "he would, but *cannot* eat." Ordered ten grains of chlorate potash to be given in mucilage of gum Arabic every two hours, "for the purpose of increasing the saline matter in the blood, and thereby increasing its capacity for absorbing oxygen from the air; and thus re-exciting and sustaining the failing organic sensibility and capillary action" to a greater degree than could be done by quinia or all the ordinary diffusible stimulants. (*Chicago Medical Journ.*, August, 1858, p. 410.) Ordered beef broth, very highly salted.

4*th*. The urine passed to day, although clear at first, yielded, after standing some hours, at least an ounce of thick, glairy sediment, like white of egg, so tenacious as to be with difficulty removed from the bottom of the vessel. Can take neither medicine nor food; "it will not go down."

5*th*. Almost pulseless; hands and feet cold; dysphagia complete.

6*th*. Is barely alive, but unconscious.

7*th*. Died last night.

Post-mortem examination, twelve hours after death.—The first thing that attracted attention upon looking into the cavity of the pelvis, was the extremely diminutive size and contracted state of the bladder. This viscus, at the time of the puncture, contained upwards of three pints of urine, it was now so small as to hold barely half a gill of liquid. This contraction—the inevitable effect of the expulsive efforts made necessary by the enlarged prostate for so many years, and which probably existed previous to the last illness—may have been overcome by the enormous distension which threatened the patient's life, and immediately returned again as

soon as the operation had relieved this distension. Great contraction and a thickening of its coats had become the natural state of the man's bladder.

From the local symptoms during life, the presumption was that an adhesion had occurred between the bladder and the walls of the abdomen, at the point of puncture; but the autopsy showed that, instead of immediate contact, there was a space of an inch and a half between the contracted viscus and the external orifice; the inter-communicating channel having become lined by an adventitious mucous membrane. Hence there seems no good reason why the canula should be suffered to remain longer than three or four days, at furthest. Indeed, it may very much be questioned whether it had not better have been entirely withdrawn at that time; and whether its presence for upwards of two weeks may not have proved a source of irritation to the bladder. In a similar case, again, I should urge its removal after the second or third day.

Owing to the condensed state of its walls, the entire mucous membrane of the bladder was thrown into many and large rugæ, and presented a dark purple colour, evidently the result of inflammatory action.

The prostate gland was very hard, and enormously enlarged. Upon slitting up the urethra back to the bladder the entire gland was exposed, and appeared of a nearly white colour; and in the extraordinary lateral hypertrophy, all traces of the middle or third lobe were entirely lost. Upon making an incision into it, its scirrhous character became apparent, feeling under the knife like gristle or leather.

The urethra anteriorly, in its membranous portion, was normal, except a discoloration—though in a less degree—similar to that of the mucous membrane of the bladder itself; and the closest examination afforded no clue to the sources of the blood which occasionally followed the attempts to introduce the catheter. But for this evidence, the occasional hemorrhage just referred to might have given rise to the suspicion of false passages, or other lesions, from the manipulations.

In the pouch of the bladder, behind the enlarged gland, some purulent matter was found; but as there was no abscess in the prostate itself, or in the contiguous structures, its existence here, or as discharged with the urine for a time, seems unaccountable; unless, indeed (although the supposition is not warranted by the symptoms during life), it may have come from the kidneys, which we were not allowed to examine. The renal source of the pus is regarded as the true one by all my medical friends who saw the case; but the examination cannot be regarded as complete, because this fact was not verified.

Bearing in mind the promising, almost convalescent condition of the patient during the last week but one of his life, when he took nourishment and stimulants so freely, with the urethra again entirely pervious, his death can only be accounted for as the result of inanition consequent on his dys-

phagia; a termination especially unfortunate, in view of his otherwise highly favourable condition. Besides, there was present an obscurely typhoid element, which complicated the case, and contributed in no small degree to its fatal termination.

The advocates of forced catheterism will, no doubt, be disposed to think and say, that the result would have been entirely different had that measure been adopted. It must be evident, however, that the violent laceration of so dense (and, in this case, sensitive) a structure, by the conical instrument, could not have proved otherwise than disastrous in the extreme; and the two lobes, in the natural situation of the parts, were so tightly in contact, that the thinnest silver probe could not be passed between them.

Moreover, if it be borne in mind that the continuity of the urethra was restored, it must be admitted that the operation itself was successful, although life was afterwards threatened, and ultimately destroyed, by causes which had no appreciable connection whatever with the local affection. However much, then, surgeons may congratulate themselves upon having never encountered the necessity of puncturing the human bladder, I feel assured that an examination of this specimen will convince every candid mind of the mechanical impossibility of introducing the catheter, in the present case, except by forced catheterism.

ART. XII.—*Case of Hydrophobia successfully treated with Drachm Doses of Calomel.* By JOHN E. H. LIGGET, M. D., of Middleburg, Carroll County, Md.

ON Monday, the 16th July, 1851, I was requested by George Mearing, Esq., of Bruceville, in this county, to visit his coloured girl, Maria, aged about 20 years, whom he supposed to be labouring under the above disease. On my way to Mr. M.'s he gave me the following history of the case: Some sixteen or eighteen days previously this girl, with his little son, eight or nine years of age, was in the yard teasing a young dog that had been unusually dull and morose for a day or two. Whilst holding her naked foot towards him, the dog snapped her in the great toe, and immediately sprang at the child, whom he seized by the arm. The girl ran at once into the house with the child, whose cries quickly alarmed the family. Upon removing the clothing from his arm, the indentations of the dog's teeth were distinctly visible, but the skin was unbroken; and as the girl said nothing of the dog having snapped her, Mr. M.'s fears were quieted. He at once had the dog chained in an out-house, where, in two or three days, he *died* with all the symptoms of *rabies canina* in its most virulent form. Some three days before I was called, Maria complained of pain in

the great toe, extending up the limb towards the body; at the same time, from being a very lively girl, she became dull, moody, taciturn, and irritable. Upon being closely pressed by her master, she confessed that the dog had seized her by the toe, and that one of the tusks had penetrated *between the nail and the flesh*, and had drawn blood. Becoming alarmed, Mr. M. went to Littlestown, Pa., to procure a nostrum (prepared by a noted empiric), which enjoys much celebrity as a *prophylactic* in this disease. Upon procuring the article he was told by the "doctor" that, if the disease was near development, she might, whilst taking the medicine, have *one or two "fits,"* which need not alarm him, as it would indicate that the remedy was producing a "proper effect." He must persevere, and she would soon be relieved of all unpleasant symptoms. Upon returning home he found the girl worse, and she now complained of pain in the epigastrium, with slight stiffness of the muscles of the neck. He gave the medicine according to directions, and sure enough, after several hours, she had a *"fit;"* after some time another, and again another. He, however, persevered until the following morning, when the medicine had all been taken, and the spasms were increasing, frightfully, in frequency and violence. He then called on me for assistance. I found her condition to be as follows: Her mind is clear, and she is conscious of the approach of the paroxysms, of which she usually gives notice. Countenance anxious and despairing; pain in the epigastrium, radiating towards the spine. Stiffness of cervical muscles increased. Urgent thirst, with *inability to swallow fluids*, which are immediately ejected from the mouth with great force. The tongue is white. Pulse 90, and rather tense. Respiration natural, except during the paroxysms, when it is hurried and laborious. There is *increased salivary secretion*, and she occasionally expectorates, with violence, small quantities of viscid mucus, which appears to be thrown from the fauces. The convulsive paroxysms are frequent, and can at any time be excited by touching her, by a current of air, or by the sight of water or other fluids.

I told her master that the disease was, undoubtedly, hydrophobia; that it had uniformly proved fatal under all known systems of treatment, and that as I proposed to pursue a course he might deem hazardous, I should prefer, before commencing it, to have my *diagnosis* fortified by the opinion of another physician. After requesting that Dr. Swope, of Taneytown, might be sent for, I scarified and cauterized the toe, directed counter-irritants to the spinal column, and left her.

5 o'clock P.M. Dr. Samuel Swope saw the case with me. We found the paroxysms still increasing. Morbid sensibility of surface excessive. Thirst so greatly increased that she now calls constantly for water, the sight of which excites great horror and immediate spasm. In the intervals complains of pain in the head. Intellect still clear. Pulse 105, tense. Heat of surface somewhat increased. After a careful examination of the

case, Dr. Swope concurred in my diagnosis. He also assented to the plan of treatment I proposed, though without any hope of averting the fatal result he anticipated. She was now bled to the amount of ℥xxxvj, and ordered hydrarg. chlor. mit. ʒj, to be repeated every *four* hours if the symptoms remain unabated. If the spasms decline in frequency and violence the intervals to be lengthened to *six* or *eight* hours.

17*th*, 8 o'clock A. M. After the exhibition of the calomel last evening she had *one* spasm, after which the spasms ceased until two o'clock this morning, when they returned with much violence. She then took ʒj hydrarg. chlor. mit., and had an enema administered (which produced but slight effect), after which the spasms again ceased. She is now, 8 o'clock A. M., lying quiet, though in other respects her symptoms are nearly as they were yesterday evening. Ordered a drachm of calomel, to be followed by an enema of ol. terebinth. Thirst to be quenched with spoonfuls of *crushed ice*, which she swallows with difficulty, her eyes being closed to avoid the sight of it. She is to be kept perfectly quiet in a darkened room, and all causes of irritation carefully avoided.

5 o'clock P. M. The bowels have been moved moderately, dejections nearly natural. She has been free from the convulsive paroxysms until within the last hour when they returned, but with less violence. ℞—Hydrarg. chlor. mit. ʒj, to be repeated in eight hours should there be any return of spasm. Continue ice *ad libitum*.

18*th*. Could not visit the patient this morning, but learned from Mr. M. that she had rested quietly since the last dose of calomel. Directed ice to be continued, and the bowels to be moved by enema, ol. terebinth.

4 o'clock P. M. No return of convulsion since last report. Bowels have been freely moved by enemata, dejections green. Pulse 108, small. Tongue heavily coated. Some heat of surface. Complains of burning pain in epigastrium with tenderness on pressure. Thirst still considerable, but dread of fluids and inability to swallow them continue. Symptoms of approaching *ptyalism*. ℞—Epispast. to epigastrium, and continue ice.

19*th*, 3 o'clock A. M. Patient had a slight spasm yesterday evening shortly after I left, which recurred in half an hour, when her master gave her hydrarg. chlor. mit. ʒss, which quieted her till two o'clock this morning, when she complained of violent spasmodic pains in the jaw and inferior extremities, when I was sent for. Ordered tr. opii ʒj, to be repeated if necessary. Repeat enema.

5 o'clock P. M. Rested from the effects of the opiate until one o'clock this afternoon, when, complaining of some pain in the jaw, she took tr. opii ʒss, which gave her speedy relief. Blister has drawn well, and greatly relieved the burning at the stomach. Mouth getting decidedly sore. She can now begin to swallow fluids, though with difficulty. Asked for food, and took a little corn gruel. As the bowels have not been opened since last evening, give her ol. ricini ℥j, to be repeated every three or four hours

until the bowels are freely moved. Repeat anodyne, should there be any nervous commotion.

20th, 2 o'clock P. M. Bowels have been freely moved, dejections dark green. Mouth deeply ulcerated but dry, and ulcers rather *livid* in appearance. Has been easy since last evening, and slept pretty well during the night. Has taken corn gruel several times to-day, and can now swallow fluids without much difficulty. Pulse 106, and weak. Exhaustion considerable. Ordered quinia disulph. gr. iij, with acid. sulph. aromat. dilut. gtt. v, every three hours. Gargle the mouth frequently with infusion of white oak bark and alum sweetened with honey.

21st, Evening. Salivary glands are discharged freely. Ulcers have assumed a healthy appearance, and she appears to be decidedly improving. Continued treatment.

24th. Has continued to improve since last report. She is now lively and cheerful. Appetite good. Evacuations natural. Mouth healing.

The subsequent treatment consisted in the regular administration of nutritious diet, tonics and laxatives, with an occasional anodyne, and she was discharged cured on the 28th.

Remarks.—I related the above case, shortly after its occurrence, to a medical gentleman residing in an adjoining county, whose venerable years and high professional attainments command the respect of all who know him. He expressed the opinion that I was mistaken in my diagnosis, and gave as his reasons—*first*, that the patient was a *female*, and, *secondly*, that the case *did not terminate fatally*. He assumed from the *sex* of the patient that it was a case of that protean disease, hysteria, *simulating* hydrophobia. He frankly added that had the patient been a *male*, or had she *died*, he would have felt himself constrained to acknowledge it to have been a case of rabies. Now, as other gentlemen may be inclined to entertain similar objections, it may be proper to endeavour to dispose of them in the outset. And first: Is it true that poisons—animal, vegetable, or mineral—have their action upon the human organization so modified by *sex* that they will produce in the *female* diseases which, although apparently identical with those excited in the *male*, are, nevertheless, mere *counterfeits*, less formidable in their nature and more amenable to the resources of science? Will the virus of the rattlesnake or viper, or the exhalations from the Pontine marshes, produce in the female affections *similar* in their *phenomena*, yet *differing* in their *nature*, from those produced in the male? The records of medicine, so far as I am aware, furnish nothing to sustain such an assumption. Why, then, shall the virus of a rabid animal produce, in the female, a disease presenting all the appalling characteristics of *hydrophobia*, but which, nevertheless, we must take it for granted is *only hysteria*, because the patient *is* a female? Such a doctrine would forever unsettle our diagnosis, not only in hydrophobia, but in *all other forms of nervous disease* in females. For instance, if a woman

receives a wound in a tendinous part, which, after a time, is followed by symptoms of *tetanus*, who shall say that it is not, in reality, hysteria assuming that disguise? More particularly should the patient recover. That, in the case I have reported, the disease was produced by a poison deposited in the wound when inflicted, will I think scarcely be doubted.

The *second* objection presupposes that hydrophobia is *essentially, and, in its very nature*, incurable. To argue that, because certain diseases have hitherto resisted the best directed efforts of medical science for their relief, they are, therefore, *necessarily incurable*, is, to say the least of it, illogical. Our want of success may, more rationally, be attributed to ignorance of the pathological conditions existing in this class of diseases, and consequent inability to employ proper therapeutic agencies for their removal. This is, unquestionably, the case in hydrophobia. Then can it be cause of astonishment that in the present state of our knowledge (or rather *ignorance*) of this malady we should be totally at fault in establishing proper indications for its cure?

The author of this paper had his attention drawn to this subject at an early period. The mysterious nature of the affection, the awful sufferings it entails, and the fearful fatality with which it seizes upon its hapless victims, invested it with an interest that irresistibly led him to seek for all the information he could obtain on the subject. After availing himself, as far as possible, of all the light that had been shed upon the subject by others, he was pained and humiliated to perceive how little had been accomplished in this particular field of pathological science.

All our assumed knowledge of the pathology of this disease is based on vague, unsettled hypothesis. Nothing has been rendered certain.

Man, as a rational being, when foiled in his efforts to unfold the mysteries of nature, is apt, nevertheless, to form certain *opinions* in regard to the matter he is investigating. These he connects into a *theory*, which, however crude and erroneous it may appear to others, *he* will hug to his bosom as TRUE until its fallacies are exposed. The writer of this article, availing himself of the labours of others—accepting such hypotheses as commend themselves as reasonable, and rejecting others his judgment cannot sanction—has constructed a *theory* of this disease. His poor efforts, humble though they be, and, perhaps, based upon error, cannot retard the progress of knowledge in a field thus far barren of results; whilst others, possessing superior facilities for investigation, may be stimulated to renewed exertions, from which the most beneficial results may flow.

Briefly, then, the views which have forced themselves on the writer, as being most in conformity with the known phenomena of the disease, are as follows:—

1st. The hydrophobic virus is an *irritant poison* whose action is directed, *primarily* and *directly*, on the great nervous centres, producing a perver-

sion of their action upon the entire organization, and thus, *secondarily and indirectly*, deranging the functions of the other organs of animal life.

2d. This virus, when deposited in a wound, remains for an indefinite period of time, locked up at the seat of injury, harmless and inert, until some *exciting cause* (of the nature of which I shall not hazard even a conjecture) occasions it to be *absorbed into the circulation*, whence it is carried to the brain and spinal cord to initiate its work of suffering and death.

3d. The primary effect of the poison on the cerebro-spinal system is to depress its action. This is succeeded by great exaltation of the sensibility and irritability of the nervous system, which progressively increases until there is a total exhaustion of the vital forces, and death results from asthenia.

4th. The dread of water or other fluids results from the suffering produced by the effort to swallow them—such effort inducing violent spasm of the muscles concerned in deglutition.

5th. The increased flow of saliva appears to be a conservative effort of the vis medicatrix to *eliminate the poison from the system* through the glands engaged in its secretion.

This is a condensed statement of the views I have been led to entertain in relation to this disease. A very few observations in explanation and support of them may be permitted me. That the action of the poison is directed primarily on the brain would appear from the fact that, in the *first* stage of the disease, the functions of the circulatory, respiratory, and digestive systems, seem but slightly, if at all, impaired; whilst dulness and pain of the head, dejection of spirits, increased irritability of temper, restlessness, unusual timidity, which causes the patient to start at every sound, with a general feeling of *malaise*, undoubtedly indicate a morbid condition of the nervous centres. That the symptoms which accompany the *second* stage (and which it is unnecessary to recapitulate) are clearly referable to morbid or perverted *innervation*, is, I presume, unquestionable. The pathological appearances after death, uncertain and inconstant as they are, bear me out in this conclusion.

Many writers upon this disease suppose that the poison is locked up at the seat of injury during the period of incubation. That it produces its morbid effects upon the system through the medium of the nerves, with the sentient extremities of which it lies in contact; and they generally concur in the opinion that it is *not absorbed*, because no traces of pain, swelling, or tenderness, exist in the lymphatics or absorbent glands. Now, if the hydrophobic virus produces its effects through the nerves with which it is in contact, how shall we account for the long period of incubation between its deposition in the wound and the appearance of the disease? Would it not be more reasonable to suppose it would *act at once* upon coming in contact with the nervous filaments, especially as from recent laceration their

sensibility and excitability would be morbidly increased? The absence of disease in the absorbent system will not militate against the doctrine of absorption if the hypothesis be admitted that the poison is an irritant whose effects are exerted *directly* upon the nervous centres, affecting the other animal functions, *indirectly*, through the perverted action of those centres. Neither will the *appearances* of inflammation, frequently found in various organs after death, prove fatal to the views here advanced. These appearances are, doubtless, deceptive, and are produced, not by *inflammation*, but by simple capillary *congestion*. The brain, pharynx, stomach, and air-passages, are the usual seats of these appearances; yet where, during life, are the symptoms that would indicate acute inflammatory action in these organs? The absence of delirium, hoarseness, cough, and dyspnœa, and of extreme gastric irritability, sufficiently refute the idea that these appearances are due to inflammation. It is more probable that the terrible muscular convulsions to which the patient is subject, by their disturbing effects upon the organs of respiration and circulation, favour the accumulation of blood in the capillaries, and thus give rise to these morbid appearances. But a more powerful argument in favour of the doctrine of absorption may be found in the fact—now generally conceded—that the virus is contained *in the saliva*, through which it is propagated from one animal to another. Now, as all the secretions are derived from the blood, how could this poison be found in the saliva unless first absorbed into the blood?

The positions assumed in my *third* proposition are, apparently, so well sustained by the symptoms of the first and second stages that to enter upon their discussion in this paper would seem to be unnecessary, involving, as it would, much useless repetition. But as differences of opinion may, and doubtless do, exist as to the immediate cause of death, a word or two in that connection may not be inappropriate.

It is contended by some writers that death is occasioned by an arrest of the respiratory processes, or *asphyxia*. That hydrophobia does, sometimes, terminate in this way in consequence of severe and prolonged *spasm of the glottis*, will not be denied. But that such is the *natural* or even *usual* manner in which the fatal result is produced, I am by no means prepared to acknowledge. That the natural tendency of great and prolonged irritation of the nervous centres is to occasion an *exhaustion of their excitability*, and consequent extinction of the functions of animal life over which they preside will, I apprehend, scarcely be controverted. That such prolonged irritation of the nervous centres does exist, in its highest form, in this affection, is abundantly manifested by all the phenomena which attend its progress. Besides, many cases are on record where all muscular commotion had ceased some time anterior to dissolution, and where death was evidently the result of a total exhaustion of the vital forces. I feel bound, therefore,

to assume this to be the *natural* and usual mode in which death is produced, unless interrupted by the casualty adverted to.

That the *dread of fluids* is occasioned by the spasm of the muscles concerned in deglutition, which an attempt to swallow them excites, is not a new opinion. It has been held by many of the ablest writers on the disease. But why *fluids* should, preferably, or in a greater degree, excite spasm than solids, I am unable to explain; unless, indeed (in the state of exalted excitability of *all the senses* during the second stage of the disease), the *oscillatory motion* of fluids, or their more *sudden diffusion* throughout the buccal cavity, should prove to be the cause. The solution of this problem, however, I leave to older and wiser heads than my own.

"The increased flow of saliva appears to be a conservative effort of the vis medicatrix to eliminate the poison from the system, through the glands engaged in its secretion."

From the novelty of this proposition (which, so far as I am aware, is exclusively my own), it will naturally be expected that it shall be supported by an elaborate series of arguments tending to sustain its pretensions as a scientific truth. Candor compels me to acknowledge my inability to do this in a manner satisfactory even to myself; and were it not that it is the *keystone* upon which my indications of cure are based, I should hesitate long before committing it, thus unsupported, to the tender mercies of the professional critic.

Yet, however erroneous it may prove to be when tested in the crucible of scientific analysis, with the results of my single successful case in view, I shall continue to cherish it *as true* until its fallacies are demonstrated. I am aware that high authorities have denied the existence of *increased* salivary secretion in hydrophobia, because no evidences of *disease in the glands* can be detected. They infer that the frothy fluid which flows from the mouth, or adheres to the lips of patients and animals, is derived from the air-passages; and refer, in support of this view, to the fact that large quantities of a similar fluid are frequently found in the bronchial tubes and air-cells of the lungs after death.

To argue that because a secreting organ may not have manifested signs of *disease*, therefore its secreting power could not have been increased, is illogical and contrary to well-known facts in medical science.

As to the assumption, that the frothy fluid which flows from the mouth is formed in, and proceeds from, the air-passages, it will, I apprehend, require more evidence than has yet been adduced in its support, to entitle it to much consideration. In all the cases of hydrophobia I have seen upon record, I have yet to observe among the symptoms enumerated, any reference to the *physical signs* indicating accumulations of fluid in the air-passages of the lungs. Surely had such accumulations existed before death, the physical signs denoting them would have been sufficiently prominent to have claimed for them especial attention. But, again, if the frothy fluid be

formed in the bronchial tubes, it must, before reaching the mouth, pass through the trachea and larynx. Could it do so without exciting violent paroxysms of spasmodic cough and dyspnœa?

Reasoning, then, from these general deductions, I assume that there *is* increased salivary secretion; that this secretion *contains the virus by which the disease is produced*, and which is separated from the blood by the secretory action of the glands. Hence, I infer that the increased action of the glands is a conservative effort of nature to rid the system of the poison. Now, if these assumptions be correct, what are the plain and palpable indications they suggest for the management of the disease? Clearly, *first*, to reduce, if possible, the extreme excitability of the nervous system, the continuance of which occasions rapid exhaustion of the vital powers and death; and, *secondly*, to follow the path *pointed out by nature*, and endeavour to aid her in her efforts to expel the poison from the system through the channel she has indicated.

But where are the remedies capable of fulfilling these indications? If we are to rely on the recorded experience of the past, we shall find this a most difficult question to answer. Bloodletting, mercury, the whole class of narcotics and antispasmodics, have in turn been tried, and thrown aside as useless. Indeed, the entire materia medica has been laid under contribution, and almost every article possessing any activity has been resorted to, with the same unvarying results.

Where, then, are we to look for an agent capable of controlling this formidable disease? MERCURY I believe to be *the* remedy for hydrophobia. But to be efficient, it must be carried to the full extent of its *constitutional powers*, by which we shall be enabled to fulfil the *second* indication I have laid down for the treatment of the disease. But the query, "How are we to accomplish the *first* indication?" naturally presents itself; and I answer, "With the same remedy—mercury!" In the autumn of 1849, I witnessed the surprisingly prompt effects of calomel, in doses of sixty and eighty grains, in arresting the spasmodic action of the muscles, and checking the vomiting and purging attending two cases of cholera which occurred in my practice—this, too, after a fair trial had been given to calomel and opium, administered every half hour, in the usual doses, and when the patients were livid and nearly pulseless. From the results in these cases, I was led to suppose that calomel, *in large doses*, might prove as efficient in restoring the equilibrium of the nervous system when disturbed, as it is in equalizing the circulation in some other forms of disease. From the notes of Maria's case, it will be perceived that the convulsive paroxysms were suspended for a period of *eight hours* after the first dose of calomel; and after the third dose nearly *twenty-four hours* elapsed, during which she was entirely exempt from them. It was not, however, until the full constitutional effects of the medicine were obtained that they entirely disappeared.

Since I commenced this paper, my attention has been attracted to a case

of hydrophobia, reported in 1811, by a Mr. Tymon, a surgeon in the East Indies, who claims to have cured the patient by *large abstractions of blood.* He says, "I began by bleeding him until scarcely a pulsation could be felt in either arm." But he adds, "Opium was afterwards given, and the patient *salivated with mercury.*" In my case, it will be observed, bloodletting was also practised, though not to the same extent as in the case treated by Mr. Tymon. I resorted to it, however, with no expectation that, of itself, it would exercise the slightest controlling influence over the disease; but simply for the purpose of obviating any tendency there might be to cerebral congestion, and for the further purpose of *promoting the more rapid absorption of the mercury,* by lessening the mass of the circulating fluids; and these, I apprehend, were the only beneficial effects produced by the large abstraction of blood in Mr. Tymon's case.

Insusceptibility to the action of medicines is a marked feature in this disease. Almost incredible quantities of opium and other narcotics have been given, without producing even the slightest degree of narcotism. This fact should be borne in mind in our efforts to bring patients under the constitutional effects of mercury; together with the additional fact that, from the rapidly fatal character of the disease, but little time is given in which to effect this object. By giving calomel in the doses here recommended (if my theory of its action on the nervous system be correct), we not only retard the fatal exhaustion resulting from the extreme excitability of the nervous centres, and thus gain time, but we rapidly saturate the system with the medicine and obtain its *specific effects* in a much shorter period than by the ordinary mode of administering it.

I have thus, as concisely as possible, stated the views in relation to hydrophobia, that have appeared to me to be most in conformity with the known phenomena of the disease, together with the indications of cure they suggest. That the whole theory here advocated may not be founded in error, I am unprepared to assert. If so, it will share the fate of all that have preceded it. But as my object in laying it before the profession is simply to invite renewed attention to a disease that, from its supposed incurability has, I humbly conceive, been too much neglected, I am willing it shall run the gauntlet of experiment and criticism, in the hope that some beneficial results may be attained.

The science of medicine, within a few years past, has, by the aid of analytical chemistry and the microscope, progressed with a rapidity unparalleled in any former age of the world. Why may not these powerful aids be invoked in the investigation of this disease? May not the subtle poison which produces it be isolated, and its sensible and chemical properties demonstrated? And might not the microscope detect lesions of structure that are undiscoverable by the naked eye?

Gentlemen who enjoy facilities for investigation not possessed by the humble country practitioner, would do well to turn their attention to this

subject. It is a goal worthy their highest ambition; for he who shall strip hydrophobia of its terrors, by demonstrating its pathology and laying down a rational system of treatment for its cure, will carve out for himself a lofty niche in the Temple of Science, and place his name among the benefactors of his race. I would rather be *that man* than to sway the sceptre of an empire.

CARROLL COUNTY, MD., November, 1859.

ART. XIII.—*On the Production of Cataract in Frogs by the Administration of Sugar.* By S. WEIR MITCHELL, M. D. (Read before the Biological Department of the Academy of Natural Sciences, October 3, 1859.)

A FEW months ago I had occasion to perform a large number of experiments upon the osmosis of woorara through animal membranes. During one of these experiments, a solution of the poison was placed within the stomach of a rabbit, and, the two extremities of the organ being secured by ligature, it was suspended in syrup. At the close of two hours a portion of the syrup, about two drachms, was injected into the subcutaneous tissues of a pigeon, who did not suffer from it in any way. A frog of small size received at the same time, in the dorsal subcuticular sac, about one and a half drachms of the suspected syrup. Much to my surprise he became feeble, and died in about four or five hours. As it was not impossible that the syrup used might contain woorara, owing to this substance having exosmosed from the stomach, the death of the frog was attributed to the poison. To correct this result, the remainder of the syrup, about three ounces, was evaporated to dryness and treated with alcohol. The alcohol was then carefully evaporated to a small bulk, and injected under the skin of a pigeon. Upon close examination it did not appear to be poisoned, or to be in any way injured by the injected material. It now occurred to me that, as the amount of sugar employed in the case of the frog was very great compared to his bulk, it might, possibly, be destructive to life when used in very large doses.

Experiment.—To test this view, three frogs of middle size were treated with sugar in the form of syrup, two drachms being injected into the dorsal subcuticular sac of each animal. Within two hours the first frog became sluggish; the dorsal sac, which had gradually enlarged, swelling from an accumulation of fluid in its interior. This fluctuating mass was the syrup, augmented by the exosmosis of fluids from the vessels or extra-vascular tissues, or both. As the frog became more and more feeble, the larger part of this collection of fluid disappeared. The frog died at the close of ten hours. During the latter portion of this time my attention was arrested by the white appearance of the frog's eyes, which, on close examination,

proved to be cataractous; the cornea remaining perfectly clear and transparent.

Experiment.—The second frog was treated with repeated doses of syrup given internally. The phenomena of exosmosis were far less marked in this case, but large quantities of mucus collected in both stomach and œsophagus, and were finally disposed of, in part, by vomiting. The mode of death did not differ from that last described. The cataractous appearance was not seen until just after the frog's death.

Experiment.—The third frog was placed in syrup, so that when seated in the usual posture the syrup covered its hind legs. Death took place in seventeen hours, but the cataract was not formed as usual, or, at least, was not externally visible. I did not examine its eyes post-mortem.

From the time I observed the development of this curious form of cataract it became a chief point of interest in the sugar poisoning, and was studied with attention. A few preliminary experiments convinced me that the best and most certain mode of causing the cataract was to inject the sugar in syrupy solution into the subcuticular sacs. The results thus obtained were extremely curious. Of eight frogs, nearly alike as to size, and all of one species (*R. pipiens*), thus treated with injections of two and a half drachms, six had cataract. In four of the six this was apparent during life, in one it was doubtful until after death, and in one no cataract could be seen until after death. Of the cataractous cases one was thus affected short of six hours, the remainder became so affected between six and thirty-eight hours. Two of the frogs suffered considerably from the poisoning, if such it may be termed; but both survived, and had no externally visible cataractous opacity. In all of these experiments the frogs were placed under bell glasses, tilted to insure ventilation, and were kept moist in each case by a piece of wetted sponge. In a second series of experiments, conducted in precisely a similar manner, it was found that when the frog died very early, as sometimes occurred, no cataract became visible. When they survived rather longer the cataract was a more frequent incident; but in a few cases no such formation took place, in despite of frequent doses of sugar to a very large amount.

It now became clear to me that I had discovered a method of producing in frogs an opacity of the crystalline lens, which might be of some value as illustrating the pathology of a subject which has always been one of extreme difficulty. So far as I am aware, no one has ever succeeded in causing cataract in the eyes of dead animals without wounding the organ, and all hope of being able to govern its synthesis in living animals has long since been given up. Many of the frogs upon whom I operated survived the constitutional effects, and remained more or less active with highly cataractous lenses. The change produced was not, therefore, of necessity associated with mortal symptoms, nor could it be regarded as a mere *post-mortem* phenomenon, since, even in the animals which did not finally survive, the lens became opaque some time before death.

With the view of ascertaining the cause of the opacity of the lens produced by the sugar poisoning, certain experiments were directed towards the determination of the effect of altering the external conditions while the frog was still suffering from the sugar. The first experiment was as follows:—

Experiment.—A large frog received under his skin about two drachms of syrup. Two drachms were also forced into the stomach through a tube, and the same amount was given in a similar manner at the close of an hour. As soon as the frog became sluggish he was placed in water and left there. He soon began to recover, and the water about him being changed thrice in the ensuing eight hours, he recovered perfectly. The dose of sugar would certainly have sufficed to destroy life had the supply of water been limited. A repetition of the last experiment satisfied me that, even with a very large dose of sugar, the animal was safe if allowed to remain in water kept fresh by frequent changes. Thus far everything pointed to osmotic changes as the probable agents in the production of the curious variety of cataract under consideration. The result of the next experiment, which in the sequence of thought naturally suggested itself as a test of this hypothesis, was such as to strengthen it considerably.

Experiment.—About two drachms of syrup were injected under the skin of a large frog. In twenty-four hours the lens was opaque, and, as the animal appeared lively, it was placed in water in order to test the permanence of the opacity. Ten hours in the water sufficed to remove most of the opacity from the lens, which began to clear in the centre first. Twenty-four hours after the frog had been placed in water the eyes were perfectly transparent, and the animal itself entirely well.

Experiment.—A distinct case of double cataract was produced in a large frog by the usual means. When the cataract first began to be visible it was placed in water. During five hours the opacity increased. In the ensuing eight hours it diminished perceptibly, but, although the water was changed twice a day, some traces of the cataract were visible during several days. The frog recovered entirely from all the effects produced by the sugar.

Experiment.—A large frog was seated in syrup. In a few hours he was nearly dead. The mucous membrane of the mouth and tongue was intensely congested, and the parts under the eye particularly so. On placing the animal in water he slowly recovered. The eyes remained clear throughout, and were not visibly affected. The congestion above referred to is a constant accompaniment of the death by sugar, but varies in degree to a remarkable extent, and does not seem to be in any way connected with the alteration of the lens.

It was found in the course of the several experiments related, when syrup was thrown into the subcuticular dorsal sac of a frog, it at first acquired increased bulk, owing to the rapid osmosis of their fluids from the frog's tissues. During this period no change occurred in the lens. As the saccharine solution became more and more diluted the current of interchange developed, and the sugar gradually soaked into the tissues of the frog, so as to be found in most of the subcuticular sacs as well as in the peritoneal cavity. As it was still possible that the original loss of water

by the tissues during the first stage of sugar poisoning, might be the cause of the cataract which afterwards formed, and, as in this case, the effects produced would be in some respects similar to rapid desiccation, the following simple test was employed:—

Experiment.—Two frogs were placed in open jars and allowed to remain without water, the temperature being from 75° to 88° F. During the experiment one frog died on the fifth day, the other on the sixth day. In neither of them was there any cataract. Both frogs were much shrunken from the loss of fluid. Mere desiccation was, therefore, insufficient to cause the opacity.

From time to time, during the conduct of these experiments, the lenses of the poisoned frogs were carefully examined by Dr. Hewson, Dr. Hunt, and myself. Dr. Hunt very kindly furnished me with notes of his observations, and, as they accord perfectly with my own results, I shall content myself with quoting his description of the general appearances presented by the cataractous lenses: "The capsule of the lens is clear, and the cells upon its lenticular surface are unaltered. The opacity begins upon the posterior face of the lens directly in the axis of vision. It is next seen on the anterior surface around a clear central spot, which corresponds to the line of cleavage between the different systems of lens fibres or tubes, the centre of the star of the lens. The opacity gradually extends all around the lens, but as yet I have never met with a case where it involved the central portions; which, on the contrary, always remain clear notwithstanding this limitation. The outside colour of the lens is often of a pearly whiteness, and the simulation of a true cataract is absolutely perfect."

When such a lens is viewed under a low power, in place of the faint indication of the track of the lens fibres which is usually seen, the line of cleavage is unduly distinct, and the fibres setting out from it are edged with dark, irregular lines, marking the interlocking with the neighbouring fibres. A good deal of granular matter is also dispersed through the preparations. In more advanced cases the fibres or tubes are enlarged irregularly, and, their interior contents escaping, are seen abundantly in the form of yellowish pellucid globules about the tubes and throughout the field of view. My friend, Dr. Hunt, and I have also observed that the same changes may be produced by soaking the eyes of frogs in syrup. By properly regulating the strength of the syrup, cataract may be thus induced without any rupture of the eyeball. I have made no experiments with larger eyes, but it is probable that, in these, also, cataract could be thus induced, and the eyes then made use of to teach the operative manual. Some such resource has long been considered desirable by teachers of ophthalmic surgery.

It may be further remarked that opacity by sugar may be produced by simply soaking the exposed lens in sugar and water. However caused, the cataractous whiteness disappears when the lenses are placed in water, but

they do not become entirely transparent where the opacity has existed for some time, or where it is very highly marked. This may be owing to the fact that in extreme cases the lens tubes are not merely altered in form, and in their relations to one another, but are also ruptured and partially emptied of their softer albuminous contents; lesions which no restoration of their aqueous supply could entirely relieve.

It appears, from the various experiments here related, that mere abstraction of water from the lens is insufficient to cause opacity; a conclusion which is strengthened by the knowledge that the exposed lens, when dried, does not become opaque. As it is found that the formation of the cataract attends the second stage of sugar poisoning, or that in which the sugar soaks into the tissues, it is probable that the direct contact of sugar with the lens is essential to the production of the phenomenon in question. That the changes which then result are osmotic seems sufficiently clear; but whether due chiefly to absorption of sugar in solution by the crystalline humour, or to exosmosis of the thinner portions of the lens fluids to the sugar, we have no means of determining. We may conclude, however: *First.* That sugar in large amounts destroys the life of the frog when given internally, injected under the skin, or thrown into the stomach. *Second.* That an abundant supply of water frequently enables the frog to eliminate the sugar and escape death. *Third.* That the formation of a peculiar variety of cataract is one of the most curious and striking symptoms attendant upon the sugar poisoning. *Fourth.* That the cataract is due to mechanical disturbances of the form and relative position and contents of the component tubes of the lens.

It is, perhaps, unnecessary to remark here that we have no knowledge of any such form of cataract in man. Notwithstanding this it would be improper to omit to state that cataract has occasionally been found to co-exist with advanced diabetes mellitus. Very recently, indeed, Mr. France[1] has reported five cases of double cataract occurring in diabetic cases. In all of these, the cataract, which was always soft, formed with great rapidity when the constitutional malady was far advanced; and in all of them the lens increased in size antero-posteriorly, and the opacity attacked portions of several strata of the crystalline humour at once, leaving clear and transparent interspaces. Now that the diabetes has any other causative relation to the cataracts in question than through the general impairment of the nutritive functions common in this disease, I do not pretend to say; but as it is possible that the long-continued presence of even a small amount of sugar in the blood may cause in the crystalline lens osmotic changes productive of opacity, I have felt it proper to call attention anew to the relation between the two maladies in question.

[1] American Journal of the Medical Sciences, July, 1859, p. 266; from Ophthalmological Hospital Reports, Jan. 1859.

ART. XIV.—*Measurements of the Diameters of the Fœtal Head at Term, collected from Seven Hundred Cases of Labour.* By JOSEPH K. T. VAN PELT, M. D.

IN calling the attention of the profession to the following cephalometrical tables, I deem it right to state *in limine*, that the most scrupulous caution has been observed to determine, with mathematical accuracy, the dimensions of the three most important diameters of the fœtal cranium.

As many of my cases have necessitated instrumental interference, from the magnitude or position of the child's head, a limited time has always been permitted to pass, thus allowing those diameters which were elongated or reduced from compression made by the forceps to assume their original extent; and the same course has also been adopted whenever in a natural labour any overriding of the frontal, occipital, or parietal bones has been observed.

The method of determining the diameters while the fœtus is still within the maternal organs, as commended by MM. Flamant and Foulhioux, and mentioned by Velpeau, proving uncertain, and their deductions being too variable to use in practice, I have employed the cephalometre of Stein,[1] its simplicity insuring a greater precision than can be obtained from any other expedient. I have also carefully preserved a written history of each of the seven hundred cases of labour that have come under my own personal attention. It is this accuracy of the deductions which gives to the record all its value and importance, at the same time it will be perceived that the details here presented are more numerous and varied than any hitherto contributed to obstetrical science.

It does not come within my province to dwell upon the importance of a knowledge of this subject, as bearing upon the judicious exercise of manual or instrumental skill, or in correcting the different aberrations of the head. If parturition were always unattended with accidents, never requiring the assistance of art, and if the fundamental principles of midwifery were not drawn from an understanding of the correspondence of the fœtal axes and those of the pelvis, then these researches would be a matter of but little moment.

It has been suggested that sex may exert an influence by increasing the bulk of the fœtal skull, and Drs. Clark, Burns, and Ryan assert that the heads of boys is from $\frac{1}{2\,5}$ to $\frac{1}{3\,0}$ of an inch larger than that of girls, while Prof. Simpson,[2] of Edinburgh, estimates the former as exceeding in its circumference by $\frac{3}{8}$ of an inch, in its transverse by $\frac{1}{8}$ of an inch, and in the inter-aural diameter by $\frac{2}{8}$ of an inch; regarding this difference as an expla-

[1] Nysten, Dict. de Médecine. Paris, 1858. Céphalomètre, p. 250.
[2] Edin. Med. and Surg. Journal, Oct., 1844.

nation of the greater frequency of stillborn males than females, we may also consider it as the frequent occasion of difficult, prolonged, and even fatal labours.

I have inserted here a collection of the longest diameters, as observed by me in the heads of both sexes; while many of them belong to females, the majority will readily be perceived to pertain to males: the occipito-mental measured $5\frac{13}{16}$, in eight males and in five females; $5\frac{14}{16}$, in four males and in five females; $5\frac{15}{16}$, in two males and in one female; 6 inches in three males; the occipito-frontal was 5 inches in twenty males and in seventeen females; $5\frac{1}{16}$, in twelve males and in six females; $5\frac{2}{16}$, in five males and in one female; $5\frac{3}{16}$, in one female; the bi-parietal measured 4 inches in twenty-one males and in fourteen females; $4\frac{1}{16}$, in seven males and in four females; $4\frac{2}{16}$, in three males and in three females; $4\frac{3}{16}$, in two males; $4\frac{1}{16}$, in one female.

My measurements of the fœtal head are the following, viz:—

I. In six hundred and forty-six crania the occipito-mental measured—

Inches.			Inches.		
$4\frac{15}{16}$	in	8	$5\frac{8}{16}$	in	115
5	in	25	$5\frac{9}{16}$	in	52
$5\frac{1}{16}$	in	20	$5\frac{10}{16}$	in	48
$5\frac{2}{16}$	in	29	$5\frac{11}{16}$	in	37
$5\frac{3}{16}$	in	29	$5\frac{12}{16}$	in	22
$5\frac{4}{16}$	in	61	$5\frac{13}{16}$	in	13
$5\frac{5}{16}$	in	56	$5\frac{14}{16}$	in	9
$5\frac{6}{16}$	in	54	$5\frac{15}{16}$	in	3
$5\frac{7}{16}$	in	62	6	in	3

The sum of which was three thousand five hundred and two inches and fourteen-sixteenths, and the mean or average five inches and seventeen-fortieths.

II. In six hundred and forty-six the occipito-frontal measured—

Inches.			Inches.		
$4\frac{1}{16}$	in	4	$4\frac{11}{16}$	in	77
$4\frac{2}{16}$	in	8	$4\frac{12}{16}$	in	81
$4\frac{3}{16}$	in	6	$4\frac{13}{16}$	in	77
$4\frac{4}{16}$	in	13	$4\frac{14}{16}$	in	76
$4\frac{5}{16}$	in	19	$4\frac{15}{16}$	in	30
$4\frac{6}{16}$	in	18	5	in	37
$4\frac{7}{16}$	in	27	$5\frac{1}{16}$	in	18
$4\frac{8}{16}$	in	37	$5\frac{2}{16}$	in	6
$4\frac{9}{16}$	in	48	$5\frac{3}{16}$	in	1
$4\frac{10}{16}$	in	63			

The sum was three thousand and thirty-three inches and fifteen-sixteenths, the mean being four inches and twenty-eight-fortieths.

III. In six hundred and forty-six the bi-parietal measured—

Inches.			Inches.		
$3\frac{4}{16}$	in	7	$3\frac{13}{16}$	in	82
$3\frac{5}{16}$	in	8	$3\frac{14}{16}$	in	72
$3\frac{6}{16}$	in	16	$3\frac{15}{16}$	in	31
$3\frac{7}{16}$	in	14	4	in	35
$3\frac{8}{16}$	in	30	$4\frac{1}{16}$	in	11
$3\frac{9}{16}$	in	47	$4\frac{2}{16}$	in	6
$3\frac{10}{16}$	in	72	$4\frac{3}{16}$	in	2
$3\frac{11}{16}$	in	78	$4\frac{4}{16}$	in	1
$3\frac{12}{16}$	in	132			

The sum being two thousand four hundred and nine inches and three-sixteenths, the mean being three inches and twenty-nine-fortieths.

Other diameters than those just mentioned have been enumerated by obstetrical authors, and they be multiplied *ad infinitum*; Churchill records seven, Velpeau seven, Jacquemier describes ten, Naegele four, and Scanzoni seven, Moreau also seven. Of the three that have just occupied our attention, different estimates have been made by writers on midwifery; the annexed table is a summary of their observations:—

	Occipito-Mental.	Occipito-Frontal.	Bi-Parietal.
Churchill,	5	4—$4\frac{1}{2}$	$3\frac{1}{2}$—4
Velpeau,	5	4	$3\frac{1}{2}$
Cazeaux,	$5\frac{1}{4}$	$4\frac{1}{4}$—$4\frac{1}{2}$	$3\frac{1}{2}$—$3\frac{3}{4}$
Maygrier,	5	$4\frac{1}{2}$	$3\frac{1}{2}$
Ryan,	5	5	$3\frac{1}{2}$
Burns,	5	4	$3\frac{1}{4}$—$3\frac{1}{2}$
Lee,	—	4	3
Ramsbotham,	$5\frac{1}{2}$	$4\frac{1}{2}$	$3\frac{1}{2}$
Moreau,	5	4 and 4 lines	3 and 8 lines
Ashwell,	$5\frac{1}{4}$	$4\frac{1}{2}$	$3\frac{1}{2}$
Murphy,	5	$4\frac{1}{2}$	$3\frac{1}{2}$
Baudelocque,	$5\frac{1}{2}$	$4\frac{1}{2}$	$3\frac{1}{2}$
Naegele,[1]	5	$4\frac{1}{2}$	$3\frac{1}{3}$
Jacquemier,[2]	$4\frac{1}{2}$	4	$3\frac{1}{3}$
Meigs,[3]	$5\frac{1}{2}$	$4\frac{10}{12}$	$3\frac{11}{12}$
My own,	$5\frac{17}{40}$	$4\frac{28}{40}$	$3\frac{29}{40}$

Prof. C. D. Meigs states that he measured the occipito-frontal of one hundred and fifty crania; in fifty-two of them he found it to exceed five inches; in 11, it was $5\frac{1}{12}$; in 8, $5\frac{2}{12}$; in 3 it was $5\frac{3}{12}$; in 1, $5\frac{4}{12}$; in 1, $5\frac{6}{12}$; in 2, $5\frac{7}{12}$; and in 1, $5\frac{10}{12}$: of one hundred and fifty bi-parietal dia-

[1] Geburtshülfe, Heidelberg, 1842, p. 64.

[2] Jacquemier, Traité d'Obstetrique, Paris, 1846, tom. prem. p. 296.

[3] "Proceedings" Amer. Phil. Soc., vol. iii. p. 127.

meters measured, sixty-eight of them exceeded 4 inches; in 19, it was 4.1; in 5, it was 4.2; in 6, 4.3; in 3, 4.4; in 1, 4.5; in only one case was it less than 3.6, the usual estimate, and in that case it fell to 3.4.

Scanzoni[1] furnishes seven diameters, viz:—

Longitudinal	1. Occ.-ment. (grosse diagonaldurchmesser) .	5″—5½″
	2. Occ.-front. (gerade durchmesser) . .	4—4½″
	3. From middle of gt. fontanel to deepest part of occiput (der kline diagonaldurchmesser)	3½—3¾″
Transversal.	1. From roots of both zygomatic processes (kleine querdurchmesser) . . .	3—3¼″
	2. Bi-parietal (grosse querdurchmesser) .	3½″
Vertical.	1. From highest point of the vault of the cranium to the anterior circumference of foramen magnum (senkrechte durchmesser)	3½″
	2. Fronto-mental (die höhe des gesichtes) .	3″

Scanzoni[1] observes that Kilian recommends us to recognize the fact, that at birth three varieties of the head are to be found, the elliptical (elliptischen form), the round (runden), and the oval (ovale form), and to class our measurements under those divisions. Accurate measurements have also been made by Dr. Addinell Hewson,[2] of one hundred and sixty-six crania: his results are occipito-mental, 5.25; occipito-frontal, 4.68; bi-parietal, 3.60. It will thus be perceived that the estimates as given by Prof. Meigs, Dr. Hewson, and myself, are far greater than those detailed by any foreign author, it then, according to Dr. Hewson, becomes a question whether the difference of the results is to be decided and explained by ethnological investigations.

I have commenced a new series of measurements, which will be published at some future period, when they have become entitled to interest by their number.

ART. XV.—*On Localized Galvanism as a Remedy for the Photophobia of Strumous Ophthalmia.* By ADDINELL HEWSON, M. D., one of the Surgeons of Wills Hospital, Philadelphia.

INTENSE photophobia constitutes the most prominent and characteristic symptom of the ophthalmia of strumous children, and is undoubtedly one

[1] Lehrbuch der Geburtshilfe von Dr. Friedrich W. Scanzoni, Professor der Geburtshilfe in Würzburg, Wien, 1853, Der Kopf des reifen Fötus, p. 88.

[2] Med. Exam. and Rec. Med. Science, vol. vii., 1851, p. 636.

of the chief causes of the obstinate and troublesome character of the diseases of the eyes in that class. The constant avoidance of the daylight, and, consequently of fresh air, both of which are so essential to overcome the peculiar constitutional taint of such cases, and the incessant fomentation to which they subject their eyes by burying them in their pillows, clothes, or handkerchiefs saturated with their tears and perspiration, for days, weeks, and even months together, not only tend to aggravate all the symptoms of the disease, but to protract them indefinitely, and, in many instances, to render the eyes incurable.

In the poor children brought to the dispensaries connected with the ophthalmic hospitals, the early relief of this symptom is of the utmost importance. Amongst them, the worst cases are those who have intemperate parents, and who are, consequently, neglected and exposed to the worst hygienic influences. For such it is worse than useless to prescribe iron, bark, cod-liver oil, and other eutrophics, to be administered at home, for much valuable time will be lost waiting for the benefits expected to be derived from such remedies, as they will scarcely ever be given. Even the injunction to keep the child's face uncovered and to give it fresh air to breathe, will not be attended to. Indeed, I am firmly convinced, from no little observation in the matter, that whatever benefit is to be derived from attempts to treat these cases at their homes, must be by remedies applied or administered by the medical attendant himself. I have, therefore, always tried by means of local applications, to afford them as early relief as possible from this photophobia, so that they would, of their own accord, seek the *light* and *air*, the best remedies within their reach at home for their recovery, and I have always found improvement in all the other symptoms to follow the relief of this one. Even in private practice, and amongst the best classes, where every comfort, luxury, and attention that wealth can command are brought into requisition, we often meet with cases which seem to receive no benefit for weeks or months, but, on the contrary, grow worse notwithstanding the most faithful and skilful use of a great variety of constitutional remedies, until the photophobia is relieved by remedies applied especially for that purpose, when the child will begin to run about, and its convalescence become rapidly confirmed. The question might be asked here, Is not the abatement of the photophobia in the latter cases rather the first sign of the yielding of the disease to constitutional remedies, or in the former the result of improved health? In many instances of strumous ophthalmia, such may be the case, but not so in the instances to which I have reference; for I refer to those in which the health was steadily failing in spite of constitutional remedies, or in consequence of the administration of such remedies being neglected, and in which the relief of the photophobia followed so rapidly the use of special remedies directed for that purpose, as to leave no doubt of the cause of such relief or of the *modus operandi* of the remedies. My impressions of the good

effects of these remedies have been so strong as to induce me to test them very critically for several years. I have thus tried blisters and tincture of iodine to the forehead and temples, vapours of various kinds (especially that of muriate of ammonia) directly to the eye, local and general anæsthesia, belladonna, and a variety of other articles. The benefits to be derived from the most, at least, of these remedies, have long been known to the profession. I shall, therefore, not comment upon them at present, but, on the contrary, confine myself to a brief exposition of the results which I have obtained from another remedy, the application of which I believe to be altogether novel. I refer to galvanism localized to the supra-orbital branch of the fifth pair of nerves, which has thus far proved to be the most efficient remedy I have ever used for this distressing symptom. I discovered this effect of galvanism during the early part of the last summer, whilst initiating some investigations on its usefulness in the treatment of ulcers, opacities, and other manifestations of impaired nutrition of the cornea, which investigations I was led to make on purely theoretical grounds of the well-known influence of the fifth pair of nerves over the nutrition of the cornea, so admirably demonstrated by Magendie many years since.

I am not aware that this peculiar effect of galvanism in the photophobia of strumous ophthalmia has ever been noticed before, nor do I know that galvanism has ever been employed as I have employed it, in the treatment of opacities and ulcers of the cornea; I therefore propose making known the results of my trials of it for both these purposes, and shall do so in two separate communications, for these two sets of symptoms of strumous ophthalmia are not necessarily dependent on each other for their intensity, and I have obtained decided benefit from galvanism in removing the physical effects of disease in the cornea where there was no photophobia present. I will, therefore, make the consideration of the latter influence of galvanism the subject of a future communication, and shall reserve for this future communication the details of the cases, even, which furnish the material of the present paper; for in all these there were lesions of the cornea which were treated after the relief of the photophobia by this same agent, and their details will be of more interest in that connection.

As it is also my desire to occupy the attention of the profession with only what has proved to be a remedy of very positive value for photophobia in my hands, I shall merely give here the results of my trials with the direct current of galvanism or chemical electricity, for I have tried the induced current of the magneto-electrical apparatus for the same purpose, but not with as satisfactory results.[1] Had the results with the induced current been the same, the intensity of the pain attending its application

[1] I may here state that I have now employed electricity in a variety of forms in the treatment of various diseases of the eyes, including amaurosis, atonic ptosis, strabismus, and mydriasis.

in the feeblest form to the supra-orbital nerve would render its use decidedly objectionable. To the direct current of galvanism there is no such objection, as the only sensation attending its application when properly localized, is that of a faint quivering or flashing of light resembling more than anything else the feeble sheet lightning of a hot summer's evening, and which proved rather a diversion than otherwise to many of my little patients.

The form of apparatus employed by me in all the experiments referred to in this paper, was the Pulvermacher's chain battery of sixty links, with ordinary vinegar as the chemical agent. The electricity from this I applied by moist conductors, which consisted of brass rods six inches long, slightly curved and surmounted at one end with a wooden handle, and at the other with a small brass cup, filled with wet sponge. These conductors were hooked to the ends of the chain, which constitute the poles of the battery, and I applied the sponge of the one attached to the negative pole to the skin over the supra-orbital foramen, whilst I made frequent contacts with the sponge of the other to the skin of the forehead at various points. These applications were generally made at intervals of three or four days, and only for a minute or two each time; and I would caution all who may resort to galvanism for these purposes not to attempt its too frequent or protracted use, or to use it in too great strength, for there is danger of permanent injury to the retina from such uses of it, as has been pointed out by Dr. Duchenne. It may, however, be used with safety more frequently than above stated. I have myself used it in private practice every day, and my sole reason for only using it at the intervals above stated, was one of convenience, as those were the intervals between my clinic days (Mondays and Fridays) at the hospital.

My reasons for employing Pulvermacher's battery were that (notwithstanding its well-known want of constancy of action), the facility of its application, and its comparative cleanliness made it far preferable for my clinical purposes to any other apparatus known to me.

In order that I might give this agent a fair trial, I suspended all previous treatment (for most had been under treatment), both local and constitutional, before beginning with it, and I avoided all cases which had been improving under such treatment.

I have thus tested it in thirty-two cases at the clinic of Wills Hospital, in which photophobia was a prominent symptom. In all these the strumous diathesis was well marked, and the part essentially diseased was the cornea. The ages of these patients ranged from one to sixteen years. Nineteen of them were females, and thirteen males. In nine, the photophobia was *intense*, in twenty-one it was *considerable*, and in only two was it *slight*.[1]

[1] As these terms are not very definite, I wish to be understood as using them in the following qualified senses, viz:—

Intense, to designate that the least light, even that of a candle, was avoided by the patient as painful.

In all but three of these thirty-two cases, there was decided and immediate benefit following the very first application of the galvanism. These three exceptional cases were all of intense photophobia. In one of them the child (two years of age) was of German parents who could not understand English, and, owing to my ignorance of German, I could not satisfy myself on their return that there had been any improvement in the case. In the second, I discovered after the child had been taken away, that my battery was not acting, and I was, therefore, prepared for the unfavourable report I got. In the third, I have no note of any circumstance by which I can explain the failure of the first application, and must, therefore, admit it as a negative result, although I believe the fault to have been in the battery. In all these three exceptional cases, there were, however, unmistakable signs of improvement on the second application.

In all the instances where benefit followed the application, it was immediate, and was generally noticeable before the child left the institution, or was observed by its parents on the way home. The benefit was also more or less permanent. I say more or less permanent, for I have notes of the fact that in two instances the children's parents did not think they bore the light as well on their way to the hospital for a second application as they did immediately on going home after the first, although there was evidently still some improvement in both the cases.

In not one single instance of strumous ophthalmia, have I noticed any bad effects, or heard any complaints of its producing any injury to the eye.

The following is an abstract of the results I have obtained in these thirty-two cases :—

Of the nine cases in which the photophobia is specified as having been intense, two (both cases of pustular corneitis) were so very much relieved by the first application, that they were out of doors the next day, playing about in the strong sunlight; three were relieved, and only one improved by the first application (this single case was relieved[1] by the second application), three (explained before) were not apparently affected by the first application, but improved by the second, and decidedly relieved by the third.

Considerable, that the ordinary amount of daylight in a room was avoided by the patient as painful.

Slight, that the patient only complained of the light when out of doors on a bright day.

[1] I use the words *improved* and *relieved* in a qualified sense, thus: *Improved* to indicate that there was a very appreciable benefit derived from the application, but not sufficient to reduce the case to the next lower of the three grades in which I have attempted to classify them as regards the photophobia; *relieved*, when such a reduction of this symptom did actually occur. To be more explicit, I indicate a case of *intense* photophobia as *relieved* when it is reduced to the state represented by the term *considerable*, but only as *improved* when the patient can bear the light of a candle, but objects to the most moderate amount of daylight; so also for the other grades.

Of the twenty-one where the photophobia was *considerable*, ten were relieved by the first application, and eleven only improved, but relieved by the second. Of the two in which this symptom was but *slight*, both were cured, one by the first, the other by the second application.

As the subsequent improvement of the photophobia in all these cases might be thought to be dependent on the abatement of the physical effects of the disease which occurred rapidly after the second application, I shall not trace this effect of the galvanism any further in these cases for the present, especially as I think I have already furnished ample proof of the good it is capable of doing for photophobia, which was the sole object of the present communication.

ART. XVI.—*Electro-Magnetism in Subacute and Chronic Rheumatism.* By H. B. TRIST, M. D., Assistant Surgeon U. S. Navy.

ALTHOUGH not unmindful of the rule which forbids that general inferences be drawn from the results obtained by any special mode of treatment in isolated cases, yet I am induced to present the following report, because, from the strongly marked circumstances attending it, the case seems to me deserving of attention.

Lieut. C. D., æt. 26, of spare habit, nervo-lymphatic temperament, good general health. Had been a sufferer from chronic rheumatism ever since the arrival of our ship on this station (the Pacific), in March, 1858. During a period of about eighteen months (in the course of which our cruise extended as far west as the Fejee Islands, returning to Panama by the way of San Francisco, where we lay some time), the affection remained unabated; remitting at times, and again recurring with considerable violence. The medicines generally prescribed on such occasions were taken during the greater part of this period; affording, however, but transient relief.

In July of the present year (1859), the patient was seized with a severe attack of lumbago, which rendered the slightest motion, even in bed, exceedingly painful. I had then recently received at Panama, among other books awaiting me on the return of our ship from the Western Pacific, a copy of the most recent of Matteucci's masterly works, his *Electro-Physiology;* in which are given his beautiful and most ingenious experiments, together with his inductions from the facts disclosed by them, in the form of laws. These experiments and laws affording scientific confirmation to my previous vague impressions, from results empirically obtained by others, in the treatment of various affections, I was induced to think that electro-magnetism (strictly speaking, magneto-electricity; the instrument I was provided with being of the latter kind)[1] might prove beneficial in the present case. The

[1] Dr. A. C. Dickinson's "Magneto-Electric Machine for medical purposes."

Besides other advantageous peculiarities—which are explained at length in the inventor's pamphlet, accompanying the machine—there is one which is not ex-

trial was accordingly made, with the laws established by Matteucci for my guide, and proceeding with the utmost caution. The current was passed from the lumbar region to the crural nerve, *placing the positive pole over the former*, so as to cause the current to flow in the direction of *the distribution of the nervous filaments;* a precaution which, though apparently of trivial importance, is of the greatest moment; since (as Matteucci's experiments prove) effects altogether different from those indicated would have been produced, had the current been passed in the opposite direction.

The first application, continued for half an hour, and of such force only as just to be felt by the patient, afforded so much relief, that he was enabled to rise, dress himself, and move about; the back still feeling stiff and painful on flexion of the body. On a second application, an hour or two afterwards, relief was so complete that the patient was able to leave the ship and take a walk of some length.

The limb having become visibly atrophied, I advised the continued use of the battery, once or twice daily, from the sacrum to the ankle-joint. In three months, not only had the pain entirely disappeared, except an occasional feeling of soreness, but the muscles had regained nearly their normal size.

This case is the more interesting, from the fact that the climate of this ocean is pre-eminently propitious to rheumatic and neuralgic affections. The patient had been more or less a sufferer during the whole cruise, up to the time when this treatment was had recourse to; since which, although

plained, or noticed, but, whether a part of his invention or not, is remarkable for its ingenuity—the very simple contrivance by which the "interruption" of the current is effected. In Matteucci's work, a highly commendatory mention is made of an "interrupter," of French invention, to which he necessarily attaches great value, his experiments proving the indispensableness of "interruption," inasmuch as the effect of the current (of any current of a given strength), depends, not at all upon the length of its continuance, but wholly upon the number of distinct shocks; that is, upon the frequency with which the current is broken or arrested, during its flow. This is owing to the fact that the action of the fluid is confined to the two instants of its entrance and of its departure. These moments—although inappreciable by our perceptions as is the interval which separated them—are yet marked by widely different phenomena.

In the pamphlet just referred to (in which the description of the machine is followed by directions for its use), Dr. D. states that the defects of plan and of construction of other machines, as explained by him, are "the main, and almost *the only reason why medical men have been disappointed*" (thus italicized by himself) "in the results desired to be obtained, in many cases where their judgment was correct." Without disputing the justness of his criticisms upon those machines, and that their defectiveness may have been the cause, in some instances, of the effect ascribed to them; one thing is certain, such disappointments can scarcely have been otherwise than inevitable, for want of the guide in the application of this powerful agent, which is afforded by Matteucci's *laws;* above all, that one of them which shows the entire dependence of the effect, in its very nature, upon the *direction* in which the current is made to flow, relatively to the nervous distribution.

still exposed to precisely the same exciting causes, he has been free from pain, requiring only an occasional resort to the battery.

In several cases of subacute rheumatism, the same treatment has been employed, and it has been attended with uniform success. In a violent attack of pleurodynia, in which topical applications had little or no effect, the pain was much assuaged by the first application; and it entirely disappeared in a couple of days.

In chronic cases, in which the use of the instrument has to be kept up for several months, it is best to suspend it for a few days, after a fortnight's application.

U. S. SHIP VANDALIA, VALPARAISO, October 11, 1859.

ART. XVII.—*Vesico-Vaginal Fistula.* By R. CROCKETT, M. D., of Wytheville, Va.

As the profession are indebted to the ingenuity and skill of Dr. J. M. Sims for the success which now attends this operation, it can be claimed as an American surgical triumph, and I look upon every successful case, now, as adding another green leaf to the wreath that entwines his professional brow.

On the fifth of November last I operated on Mrs. U———, of this county, for vesico-vaginal fistula, a brief report of which I give below.

Mrs. U., aged thirty-two years, was delivered of her first child about three weeks before I was called to see her. Her labour was protracted and painful, and rendered more difficult from a deformity of pelvis, resulting from an unnatural direction of the right ascending ramus of the ischium, contracting the space of the inferior strait so much that embryulcia had to be resorted to. A speedy delivery was rendered the more necessary from the occurrence of a puerperal convulsion a short time before.

Examination.—Found a fistula of sufficient size to admit the forefinger, situated in the right side of the vesical trigone, which she assures me was produced some two months before her confinement, one day whilst weaving. It must have resulted from the lodgment of a calculus there. She has since discharged small lithic acid calculi.

Operation.—Assisted by my son, Dr. Joseph Crockett, she was placed in the usual position directed for that operation. No anæsthetic was administered. After the introduction of the lever speculum the edges were *freely* pared, the silver sutures introduced and clamped as directed by Dr. Sims. When the ends of the sutures were cut, I bent their ends down and pressed them into the shot with the forceps, so as to prevent any irritation.

The patient was placed in bed on her back, in which position she remained, and the self-retaining catheter introduced. Her bowels were con-

stipated by the administration of half a grain of opium every eight hours. Her diet cream and thin crackers with a little coffee, to have water freely when thirsty.

There was no departure from the recommendation of Dr. Sims either in the operation or after treatment, except in this. The doctor says "the patient's comfort is greatly promoted by washing the vulvar opening twice a day or oftener with warm or cold water, as may be preferred."

"For this purpose a common bed-pan is placed under the nates as she lies on the back; when the water may be thrown into the os externum, over the mons, vulva, and inguinal regions by means of a syringe holding some six or eight ounces. The water has to be thrown with considerable force to remove the urinary deposits from the nates and genitals."

I placed an oblong piece of oiled silk under the end of the catheter, pressing a fold gently between the labia, then folding the lower end in such a manner as to convey the urine as it escapes from the end of the catheter into a small, shallow vessel, tapering nearly to a point, placed between the thighs. If necessary the piece of oiled silk can be retained, by fastening a piece of tape to each of the upper ends, and attaching these to the abdomen by small adhesive strips. On the third day an examination was made and I found the sutures all doing well. On the eighth day the union was so perfect, that I removed the sutures, the cicatrix presenting a smooth and beautiful appearance.

The patient was replaced in bed and the recumbent position maintained for a week, and the use of the catheter also.

The bowels were moved on the tenth day by the administration of an enema of flaxseed mucilage. In the mean time not the slightest inconvenience resulted from constipating them that length of time.

The operation was performed eight weeks after she was delivered.

ART. XVIII.—*Tracheotomy for the Removal of a Persimmon Seed.* By E. MASON, M. D. Wetumpka, Alabama.

ON the 11th of December, 1858, Mr. G. L., of Coosa County, brought his little son to me, and gave the following history of his case: The little fellow, seven years of age, while attempting to bend down a sapling, clinging to it with his hands and feet, his back towards the ground, was taken suddenly ill; his father, who came immediately to him, finding some persimmon seed in his mouth, suspected the difficulty, and sent for Dr. McKenzie, who lived close by. When the doctor arrived, he at once recognized a foreign substance in the trachea, and advised the child to be brought

to me for its removal. The accident occurred in the forenoon, and I saw the patient about twilight, by which time the urgent symptoms had passed off. His breathing was then croupy, cough troublesome, his countenance bore rather an anxious expression, and during the paroxysms of coughing, the foreign body could be distinctly heard passing up and down the trachea; but still these symptoms did not seem threatening, and he was comparatively comfortable.

The following morning he was seen in consultation with Drs. Moore and Harris, and the presence of the substance in the trachea gave so little inconvenience, that it was agreed that the little patient should be sent home, hoping the seed might be expelled during a paroxysm of coughing; but should he be threatened with suffocation, or should constitutional disturbance give warning of danger, his father was directed to give immediate notice.

From this time until the 15th he suffered very little inconvenience, except not being able to rest well at night. On that day he had some fever, which increasing on the 16th produced slight spasms. We were summoned to see him on the 17th, and found him pale, with a wild expression of countenance; there was considerable bronchial irritation, and the stridulous breathing was more aggravated, the paroxysms of cough were more severe, and his nervous system was considerably disturbed. The local symptoms and the constitutional irritation warned us that it was unsafe to wait longer for the expulsion of the foreign body by coughing.

Assisted by Drs. Moore, Harris, and McKenzie, I proceeded to perform tracheotomy. We attempted to put the patient under the influence of chloroform, but the feeling of suffocation became so distressing that he begged not to breathe it, urging that he would hold perfectly still, which promise, much to our astonishment, he faithfully kept. Commencing at the inferior border of the cricoid cartilage, I made an incision downwards nearly to the top of the sternum, and dissected carefully down towards the trachea. The thyriod vessels and the isthmus of the glands were pushed aside with the handle of the scalpel, and the dissection completed with the loss of but little blood. After waiting a few minutes for the slight hemorrhage to cease, I divided three rings of the trachea, and had the gratification of seeing the persimmon seed immediately expelled, with considerable force, through the opening, during an effort to cough. The wound was closed in the usual manner, and cold water dressing applied. He was given a mercurial, and put on small doses of tartar emetic, with opiates when necessary, to quiet his cough, or to procure rest. On the 19th air ceased to pass through the opening in the trachea, and the wound healed rapidly.

On the 20th the bronchial irritation and the febrile excitement subsided, and in a few days the little patient was well again.

In the September No. of the *New Orleans Medical and Surgical Jour-*

nal, Dr. Stone, after reporting a case of tracheotomy for the removal of a grain of corn, appends some excellent remarks in favour of immediate action, where it is ascertained that foreign bodies have lodged in the trachea. Although the case above reported did well after waiting six days, yet I am satisfied that the delay subjected the patient to great risk, and under similar circumstances I should operate immediately. The danger of sudden suffocation, and of grave local and constitutional irritation from the presence of a foreign substance remaining in the trachea, overbalance the possibility of its being expelled during a paroxysm of coughing.

ART. XIX.—*On Incurved Toe Nail.* By ISAAC G. PORTER, M. D., of New London, Conn.

HAVING myself been a sufferer (ὁμοιοπαθής[1]) from this annoying affection, I can speak feelingly on the subject, and I am induced to record my experience, personal and otherwise, in regard to it, in the hope of affording relief to others similarly afflicted.

This affection arose in my case, as is usual, from wearing too short a boot, by which the dorsum and edges of the nail were pressed downwards, and the integuments upwards, for want of room. At the time of its occurrence, I was visiting similar cases, and, it being easier to manipulate on others than on one's self, they escaped with only slight inflammation and suffering, but before I was aware, profuse granulation had occurred in my case, and months elapsed before the annoyance ceased. It is well known how surely and rapidly recovery ensues in such affections, when from any cause, such as a fever, the patient is compelled to cease walking, and to recline in a horizontal position; but the problem is, as in this case, how to be restored, without relinquishing active exercise. Treatment was attempted by endeavouring to remove pressure, and drawing the soft parts from contact with the nail by adhesive plaster, at the same time interposing lint. Inflammation and suppuration, however, having occurred at the outset, the integuments being thereby thickened and enlarged, and the granulations profuse and irritable, the edge and corner of the offending nail could not be removed. Caustics and the knife were frequently used, but no permanent

[1] Sydenham inculcates the beautiful sentiment, that "the physician ought seriously to consider, that he, himself, is not exempt from the common lot of mankind, but is subject to the same laws of mortality, the same accidents, and the same sorrows as others; therefore, let him, *fellow sufferer* (ὁμοιοπαθής) as he is, with greater diligence and love, seek to relieve the sick and the afflicted."

benefit resulted. The seat of the affection was, as is usual, the outer corner of the large toe.

The points subsequently developed were the following :—

1st. The importance of removing *all* pressure, even the slightest, from the irritable granulations, an elastic stocking being sufficient to perpetuate the mischief. This becomes especially manifest in drawing on a boot, and until the evil is in some way abated suppuration and granulation will continue. This was effectually accomplished by the following procedure, a course which it may be prudent to adopt even in mild cases where the evil has previously existed merely as a preventive measure. After a loose stocking had been drawn on the foot a friend was requested, having pulled its extreme end forward, to stitch a seam partly across it in front of all the toes except the large one, thereby hanging or resting the stocking on the second and third toes, leaving the affected phalanx in a sulcus. Thus all pain was prevented even in drawing on a boot. But not even a buck-skin shoe could be worn. Hence a circular piece of leather from a partly worn boot the size of a quarter dollar was removed directly over the affected nail. To avoid attracting notice when abroad, a small piece of black silk was tacked on the stocking beneath the circular opening in the boot, which prevented nearly all observation and inquiry. This method is much more effectual than the crucial and other incisions which are sometimes made.

2d. The point is mooted whether, in such cases, it is proper to remove a portion of the nail, some affirming that, as it will soon grow out, it will ultimately do no good, preferring, as they say, to let the corners of the nail grow to considerable length; while the middle of the dorsum is kept very short, and shaved thin. This opinion is doubtless founded on a misnomer in the title of this affection, and a mistake as to its true cause. "A name often blinds us to facts." It is not affirmed that there may not be cases where, from mechanical injury, or a diseased condition at its base, the nail may not be incurvated; but, in a very large proportion of cases, the disease is in the soft parts, and arises from pressure. Where the inflammation is slight, or even where suppuration has occurred, if the corner of the nail can be skilfully removed, the trouble ceases, as certainly as when a thorn is removed from the flesh, provided all future pressure is prevented; and, long before the nail grows out, the parts will have healed. Having never met with a true incurvated nail, the writer has nothing to offer in regard to its proper treatment.

3d. If granulations have in any case freely sprung up, and have effectually concealed the corner of the nail which is irritating the swollen integments, then it is doubtless proper to foment and poultice the part, that inflammation may be allayed, and an attempt made to remove a portion of the nail. If unsuccessful, then the choice lies between the knife and caustic, and, if the latter, the question arises which one is the best. In the present case, the knife was used by a friend, as were nit. argent. and exsiccated

alum; but the granulations returned, possibly because *all* pressure was not removed. Subsequently, resort was had to the method mentioned above for relieving even the weight of the stocking, and, soon after, to a hot, saturated solution of alum, as recommended in this *Journal* (July number, 1854, p. 246) for obstinate ulcers about the toe nail. It was applied with a hair pencil as hot as could be borne, following which, small pledgets of lint, wet with it, and covered with oil-silk, were applied. After twenty-four hours, dry lint was substituted, wet and dry applications being alternated. Under this treatment, which was not painful, the granulations flaked off, or were absorbed, and, at the end of three days, were so far diminished that the macerated corner of the nail was easily removed. Many months have since elapsed, with no return of the annoying evil.

The hot, saturated solution of alum acts kindly as a mild caustic. Why may it not be of use, generally, in spongy granulations of small extent? Being applied hot, in order to manifest its greatest efficiency, may be an objection in irritable surfaces of much extent. Holding in solution, as it does, an excess of the salt, this excess is deposited, on cooling, in an impalpable powder; and, in small granulations or ulcerations, particularly about the nails, its action is unique and salutary.

REVIEWS.

ART. XX.—*A System of Surgery; Pathological, Diagnostic, Therapeutic, and Operative.* By SAMUEL D. GROSS, M. D., Professor of Surgery in the Jefferson Medical College of Philadelphia, &c. &c. &c. Illustrated by nine hundred and thirty-six engravings. In two volumes. Philadelphia: Blanchard & Lea, 1859. 8vo. pp. 1162 and 1198.

AN American systematic work, embracing the whole theory and practice of surgery, is well calculated to command attention, even without the prestige of name and imprint which belongs to the one which we are now about to notice.

There has been no lack of admirable works on other branches of medical science; many of these are well known, and we might point to them with reasonable pride, as affording ample evidence of proficiency in more than one department of medical instruction. Nor, in spite of much vociferous croaking, is our surgical literature deficient in clinical records, practical essays, elaborate monographs, and really valuable treatises on most of the important surgical questions. It is in elementary teaching, more than in actual working progress, that our writings on surgery have been in the background; in the absence of text-books of sufficient scope and weight to compete with such manuals as the lectures of Erichsen, the Principles and the Practice of Miller and others; in the non-existence, too, of works of higher grade to take the place of the yet unrivalled productions of Chelius, Vidal (de Cassis), Boyer, and others of similar extent and character.

There is no doubt as to our decided independence in the practice of surgery—its many admitted triumphs and unaided steps in advance; but in spite of its popularity, and the well-earned distinction of so many of its teachers, our young men have been compelled to have recourse to foreign sources for too much of what they ought to have been provided with at home. The reprints, so much complained of, are the natural exponents of an urgent want, and are seen in our students' hands solely because so few satisfactory substitutes have been put within their reach by those who ought long ago to have met the evil, not by complaining of it, but by just such an effort as is embodied in the portly volumes now before us. This is a beginning in the true direction which every right-minded member of the profession will gladly welcome, because it is not only legitimately but ably made; it deserves success, and will obtain it, from the simple fact that it is the very first treatise, on the entire subject as taught and practised, that has yet been published in the country.

Our epitomes, outlines, compends, lectures, and other more ambitious essays, however useful in their way and time, are, with one or two exceptions, either too much out of date or too incomplete to be preferred by learners to the recent, more convenient, and more instructive European class-books; while they are, moreover, too far behind the colossal treatises

of Chelius, Vidal, Boyer, Velpeau, and many others, to satisfy the more advanced inquirer.

Our author has endeavoured to combine, more or less, the objects of the two classes of instructors; and thus, in some measure, to provide a succedaneum for this too exclusive reliance on foreign contributions, by furnishing to both classes of readers a standard system of surgery, which, while claiming a basis of individual experience acquired at home, is intended to approach, in erudition, the range and level of the European masters. His aim "has been to embrace the whole domain of surgery, and to allot to every subject its legitimate claim to notice in the great family of external diseases and accidents." With such competitors before him this is certainly a bold attempt, and deserves a cordial acknowledgment from all American readers, were it only for the example's sake, whatever the success attending it may be. Indeed, it is impossible to view the result of an enterprise so honourable to all concerned, in these full, well printed, and beautifully illustrated volumes, without a strong feeling of respect; and we doubt not that the ability, industry and confidence in the appreciation of the profession which have led to their completion in so creditable a form will be abundantly requited by their speedy circulation.

How far the main object of the publication has been accomplished in meeting the wants of American readers, must, of course, remain a matter of opinion, which it is a part of our present duty to discuss. Could we be allowed to indulge in the more agreeable office of congratulating the author and his brethren on the happy fulfilment of his task, we might content ourselves with a brief account of the general arrangement and of some of the more salient features of the work. But we cannot forget the heavy obligation resting on us in expressing our opinion upon the crowning demonstration of one whose position and reputation give great weight to his words, and who writes as if he deeply felt the force and responsibility of his double vocation as a teacher at home and a representative abroad. In the inevitable sense of accountability to a common tribunal as his critical sponsors, must he find our apology for some occasional strictures, in regard to his manner and matter, which might not be thought worth noting in the review of an inferior book, or of a less prominent author.

His work is undoubtedly the best of its kind that has yet been produced in this country; and in many respects it is equal to those of a similar class of European publications; while, in extent of scope and fulness of discussion and illustration, it is superior to some of the most popular. Nevertheless, we should be sorry to see it generally regarded as an adequate exemplar of the present state of the surgical science and art of this country. It has some peculiarities which, by giving it a stamp of greater independence and originality, and adding somewhat to its individual character, may exalt it in the estimation of a certain description of admirers; but, for all truly practical intents, many of these oddities are only marks of weakness in a work which is designed to take a national position, and to retain a permanent hold upon students and practitioners as a classical authority.

We feel some embarrassment, in making up our estimate of its destined position, from the difficulty in determining in what light we are to consider it; whether as a text-book for students and junior practitioners, or as a book of reference for practitioners alone. It must be intended as the companion-book to the author's lectures, and hence to be diligently conned by a large body of embryo surgeons; and yet it is dedicated and addressed espe-

cially to those who have already begun their surgical career. It is offered, not as an introduction to the more extended study for the use and assistance of his and other pupils, but "as a systematic and comprehensive treatise on the science and practice of surgery considered in the broadest sense; one that shall serve the practitioner as a faithful and available guide in his daily routine of duty."

The important distinction involved in this ambitious undertaking to supply the office of a surgical library as well as of a daily practical adviser, cannot be misunderstood by the reflecting reader. The undergraduate may make the most of the aid and comfort that is proffered to his seniors, although the volumes may not suit his convenience, in size and form and elementary detail, so well as some other less cumbrous and more compendious favourite of his early readings; and we hope he may, as many doubtless will, prefer their teaching as his ruling monitor, until he is properly prepared for a wider range of study; but the intelligent practitioner would hardly rest content without something more comprehensive and more minute in practical directions, if not more profound in theory, in a work of last resort. However important it is to secure to the beginner a solid groundwork for his later progress, no one would expect the same thoroughness in the preparatory course, that is essential to the guidance of the subsequent practitioner; and hence we might willingly pass by sins of omission and commission, in the lessons of a teacher intended for his pupils, which could not be left unnoticed in the standard guide-book, the precepts of which are set down as virtually beyond appeal.

Looking at the treatise of Professor Gross from the higher stand-point which he seems to have elected, we are not prepared to boast of it as a sufficient substitute for the larger and fuller works of similar pretensions which have come to us from the European press. We have no idea that it so satisfies the author himself, now that he can make the comparison; and we believe that he will be among the first to desire either to condense it for the humbler but not less useful function of a text-book, or to raise it, as he can and we doubt not, will do, in future editions, to the more elevated position which it ought to occupy. It is because we earnestly desire and expect this ultimate success that we are not disposed to withhold a present expression of regret that he is not yet within the goal to which he should eventually attain for the honour of the American profession.

We confess to a dread of these attempts to crowd into the narrow compass of one or two volumes, however bulky and small in type, the contents of a working library, or anything beyond the sphere of an introductory manual. The man of only one book can never be a safe guardian of the health and physical integrity of his fellow-men in disease or injury. We strongly doubt the possibility of cramming into a work of two volumes, albeit scarcely portable, a detailed exposition of the science and art of surgery—however concise and free from extraneous matter it may be—which would not fail to be a satisfactory adviser to the inexperienced practitioner, under even the ordinary contingencies of daily practice. We do not know of such a feat of book-making in any country, although instances might be shown, in different languages, of the necessity for greater space, in cases where every page is densely filled with strictly essential matter, and where the pages greatly exceed in number those of our author.

Nothing short of an encyclopedia can adequately do this service; and no less extended work can safely serve the purpose of a generally available reminder upon the ever-varying multitude of topics and questions, even with

a compactness of arrangement and a brevity of language and discussion which are not predominant characteristics of the present treatise.

Under this view of the case we would expect, in an encyclopedic volume of not over twelve hundred pages, the largest amount of positive knowledge in the smallest possible space; but, unfortunately, this expectation is not gratified. It is true that, along with much that needs pruning in language, tone, and style, there is a great deal that is unexceptionable in these important requisites. Some passages are models of clear, correct and terse description; others again are as entirely the reverse, being diffuse and magniloquent in style, and not very sparing and choice in the use of adverbs and adjectives.

In spite, too, of his own warning in the preface, we are struck, in looking over the chapters, with the scarcely avoidable inequality of distribution as to topics, which has justly troubled him in other writers. A greater interest in subjects with which he happens to be most familiar, has, as usual, beguiled him into an over-estimate of their relative importance; while in a few instances an undue prejudice has evidently produced the opposite effect.

The article on "Tuberculosis of the Joints, especially as it affects the Hip," although by no means exhaustive on the subject, occupies twenty-six pages; that on neuralgia, one of doubtful surgical interest, ten pages. On paralytic affections there are six, and on the teeth, four pages; while diseases of the ear are dispatched in twenty-nine pages, and diseases and injuries of the eye in one hundred and nine. The chapter on the eye is not much more than four times as long as the section on hip-disease, and but little over twice the length of that on injuries and diseases of the head. We do not mean to say that either of these articles is unreasonably long, or that the subjects generally are not fairly discussed; but we do protest against the superficial manner in which he has treated the diseases of the eye, the unjust sneer at specialists, which disfigures a prefatory page of idle comment, to the contrary notwithstanding. It might easily be shown how the material of this chapter bears the refutation of its author's erroneous estimate of his difficult subject in its own striking insufficiency. We shall not, however, waste time in attempting to enlighten him upon a question which is too well settled among the highest surgical authorities not to render the bad taste and confused verbosity of this unfortunate passage a gratuitous injury to the credit of the book. Errors of omission, however, are not the most obvious short-comings of the work; a greater defect, especially in a text-book for students and young practitioners, is its diffuseness in many places, and the manner in which, by episodical discussion, often declamatory, the bulk of the volumes is increased without any compensating practical advantage. Their practical usefulness would be greatly augmented, if the space lost in figures of speech and various critical demonstrations in regard to matters of opinion, which might often be disposed of more agreeably in milder language, were occupied with specific references to the authorities named, and with others which might be cited for the benefit of students and professional inquirers.

We believe it to be true, as our author affirms, "that there is no topic, properly appertaining to surgery, that will not be found to be discussed, to a greater or less extent, in these volumes;" and we fully agree with him in the propriety of devoting "a larger space than is customary to the consideration of inflammation and its results, or the great principles of surgery," and of bestowing elaborate attention "upon the discrimination

of diseases," and "on general diagnosis;" but, considering the paucity of references and the absence of many practical details, we do lament that all of this and much of his other matter, were not condensed into a more available compass.

No properly informed reader can doubt the weight of authority which necessarily attaches to the dictum of Professor Gross; and all, of course, will regard these volumes in the light in which he offers them, "as embodying the results of a large personal, if not of a ripe, experience, of extensive reading, and of much reflection; in a word, as exhibiting surgery as I myself understand it, and as I have for so many years conscientiously taught it." Still, in glancing over the many hundred pages, and especially in resorting to his various chapters for the investigation of many difficult questions, it is natural to expect, in such an amount of matter, more definite assistance toward the further prosecution of the study, in the records of the labours of the master minds which he has consulted in the extensive reading, which is one of the qualifications set forth in support of his authority as a teacher. We cannot accept the dogmas of any one writer as final, least of all in a book of general reference; and we should have little confidence in the student or practitioner who could be satisfied with such a narrow limit to his preparation for the grave and ever-changing responsibilities and contingencies of his important calling. The value of "a faithful and available guide to the practitioner"—is surely quite as much enhanced by the documentary and specific evidence it affords of attentive examination of other sources of information, as by the highest authority that mere reputation for experience can establish; nor will any amount of personal influence or asseveration, which is not strengthened by full and explicit citation of standard works, be likely to establish the position of a systematic text-book.

Notwithstanding these and some other discouragements at first view, we have been agreeably disappointed in many parts of these volumes, after a prolonged and conscientious examination of their contents; being well satisfied that the author has succeeded in overcoming many difficulties, inseparable from his task. It is a good sign that a better acquaintance with the book increases our regard for it as a whole. The more we read of it the better we like it, in spite of its numerous minor blemishes, and a few greater defects. An extended analysis is out of the question in a review of a systematic treatise. It will be sufficient for us to give a brief survey of the general arrangement, and a slight running commentary upon a few among many things which have attracted our attention in our inspection of the chapters.

The whole subject is treated under the two heads of General Surgery and Special Surgery. The first of these occupies 642 pages, and the second some 1627 pages, 36 pages being devoted to a full and carefully prepared index. The first part, or General Surgery, is divided into nineteen chapters, which treat of irritation; sympathy and idiosyncrasy; congestion; inflammation; terminations and results of inflammation; textural changes; congenital malformations; tumours or morbid growths; scrofula; wounds; effects of injuries upon the nervous system; syphilis; general diagnosis; minor surgery; operative surgery; plastic surgery; subcutaneous surgery; amputations in general; excision of the bones and joints; and lastly, anæsthetics, or the means of averting pain.

In the second part, on Special Surgery, or the diseases and injuries of par-

ticular organs, textures, and regions, we have twenty-eight chapters, of which the first eight are in vol. I., and the remainder in vol. II. The eight chapters in vol. I. are occupied with diseases and injuries of the skin and cellulo-adipose tissue; of the muscles, tendons, bursæ, and aponeuroses; of the lymphatic vessels and ganglions; of the nerves; of the arteries; of the veins; affections of the capillaries; diseases and injuries of the joints. The twenty chapters of vol. II. present us with diseases and injuries of the bones and their appendages; of the head and of the spinal cord and column; diseases and injuries of the eye; diseases of the ear; injuries and diseases of the nose and its passages; of the air-passages; of the neck; of the chest; of the jaws, teeth, and gums; of the mouth and throat; hernia; diseases, injuries, and malformations of the anus and rectum; wounds of the abdominal organs; diseases and injuries of the male genital organs, of the female genital organs; special excisions of the bones and joints; special amputations; affections of the extremities.

A better idea may be formed of his mode of arranging his subjects by the following extract from his preliminary observations, being the first two paragraphs:—

"Under the division of GENERAL SURGERY, I shall consider, at some length, the leading facts illustrative of the great principles of surgical disease and injuries, and of the operations necessary for their relief, removal, or cure. Beginning with the subject of irritation, sympathy, and congestion, as intimately concerned in the production and maintenance of many of the most common and interesting morbid phenomena, I shall pass successively in review all that relates, generally speaking, to inflammation and its effects; textural changes; new formations, whether benign or malignant; wounds or poisons; as all these affections are liable to occur in every organ, tissue, and region of the body. To this division of the book, also, naturally appertain the subjects of general diagnosis, surgical instruments, and other appliances; and, lastly, anæsthetics, or the means of averting pain during the performance of surgical operations, and in reduction of hernia, fractures, and dislocations. Such an arrangement is suggested not less by common sense than by sound experience; for it is just as necessary in surgery to pass from the known to the unknown, or from the more simple to the more complex portion of the various topics which fall within its province, as it is in the study of mathematics, or any other abstruse science."

"The above subjects being disposed of, we shall come to that of SPECIAL SURGERY, embracing an account of the various surgical diseases, injuries, and malformations of particular organs, tissues, and regions of the body. The general principles, or great doctrines of surgery being understood, a knowledge of the various topics discussed in this part of the work will be a comparatively easy task."

The commencing chapter on irritation, sympathy, and idiosyncrasy, is interesting and suggestive. It must prove particularly useful to beginners, for whose benefit we hope to see it considerably extended in the next edition. The two chapters, which, with a short one on congestion, follow next, are long and full of interesting matter. Although peculiar, on certain points of doctrine, they are generally well arranged, clear, and to the purpose; and present an instructive, fair, and practical view of the present state of all that is really essential in our knowledge of inflammation and its results. We are glad to find this very important portion of a systematic treatise as well brought up and as free from special views and speculations as could be expected in an exposition of the kind. The sections on the treatment of inflammation and of its results, may be recommended as especially reliable, whatever may be thought of the occasional abstractions and old-fashioned pathological views in the doctrinal discussion of the subject.

We had marked many passages in these chapters, as elsewhere in the volumes, for inquiry or approval, but prefer to quote an extract, as of more use than critical discussion, since it affords some idea of our author's style, and his mode of reasoning on a knotty and hackneyed subject.

"Much discrepancy exists among writers and teachers in regard to what constitutes inflammation; some, among whom I include myself, believing that but a slight degree of action is necessary, while others maintain that the departure from the healthy standard must be very great. Thus Dr. Miller, Professor of Surgery in the University of Edinburgh, declares that true inflammation, properly so called, is always attended with suppuration; apparently forgetting that thousands of human beings daily die from this affection long before it has attained this crisis. Dr. John H. Bennet, another eminent Scotch professor, makes fibrinous exudation the indispensable condition of the process, and he goes so far even as to propose the word exudation as a substitute for that of inflammation."

Objecting to the term as ill-chosen, because likely to confuse the student's mind with an erroneous idea, he continues—

"Dr. Bennet, and those who think and reason with him on the subject, must be aware that there are inflammations of certain organs and tissues in which the morbid action is so great as to destroy life, and yet the most careful examination, microscopical and chemical, fails to detect the existence of fibrin in the affected structure. It is only necessary to instance the arachnoid membrane, the aponeuroses, cartilages, and nerves, in which this disease is often, if not generally, unattended by a deposition of fibrin. But while it is certain that inflammation is frequently present, and that, too, to a serious extent, without fibrinous exudation, it is equally true that this substance is usually poured out in this disease, especially if it has already made considerable progress. Much will necessarily depend upon the nature of the affected organs and tissues, some furnishing plasma much more readily and in greater quantity than others. Moreover it requires very nice judgment, particularly in the living subject, to define the boundaries between congestion and inflammation, or to determine where the one terminates and the other begins. Inflammation, in its inceptive stages, may be compared to a latent or smothered fire kept in abeyance by a redundancy of surrounding material interfering with its development; exudation cannot occur all at once; some time is necessary to prepare the vessels for their new office: so it is with the flame in the furnace; it does not break forth immediately on the application of the kindling, and yet no one would say that fire was not actually present. Most of the disputes that have grown out of this question have arisen from a misunderstanding on the part of observers as to the amount of disease, or change in the affected part, necessary to constitute inflammation; and it is obvious that there never can be any fixed or settled views upon the subject so long as this is the case; nor can the question be satisfactorily disposed of, unless it be studied with reference to the nature and functions of the different organs and tissues of the body; or, if I may use the expression, the conduct and habits of the organs and tissues in their healthy and morbid relations."—pp. 96, 97, vol. i.

Passing over the remarks on textural changes and congenital malformations, we come to Chapter VII., of sixty-six pages, on tumours or morbid growths. This difficult subject is well managed so far as the detailed accounts of special tumours are concerned, these being carefully described and very well illustrated. The directions as to treatment are judicious and valuable, including, among other precepts, a caution against the free use of the exploring needle in the diagnosis of suspicious growths. Dr. Gross objects to this mode of exploration, as liable to arouse injurious irritation without affording a compensating amount of information as to the nature of the tumour thus explored. His views in regard to extirpation are

already well known. His "conviction is, that interference with the knife," in carcinomatous disease, "is, as a general rule, only productive of harm, and that the patient will live quite as long without as with it, and, on the whole, in a state of greater comfort." Seven "general rules for conducting excision of malignant diseases," and seven others in which are described "the contra-indications to surgical interference," afford invaluable assistance to the doubting operator, and may be consulted with advantage by surgeons generally. The classification of these tumours under the uncertain heads of benign and malignant, is not so happy; nor are the general descriptions of these two arbitrary divisions more satisfactory. Our author seems to have much more faith in the old-fashioned distinctive signs of cancer than in the microscopic means of diagnosis. He regards this instrument as "often a valuable auxiliary, but nothing more."

"The practical surgeon," he continues, "must indeed be dull who cannot, as a general rule, determine the character of a morbid growth before he attempts its removal. The merest tyro in the profession has no difficulty in discriminating between a gelatinoid and a fibrous polyp of the nose, a scirrhous and an encephaloid tumour of the breast, or a common hypertrophied ganglion of the neck and a malignant growth of the same part. Every new growth, whether benign or malignant, has its peculiar features, not less than every pre-existing one. I believe that, with proper care, and a reasonable knowledge of the morbid structure, such as every cultivated surgeon ought to possess, it is, in general, as easy to determine the difference between a malignant and a non-malignant tumour as it is to determine the difference between a muscle and a tendon."

He does not wish to "underrate microscopical researches; on the contrary, I only wish to state that they have not, in my judgment, effected all the good that has been claimed for them, especially in this particular department of pathology and practice, and that therefore their results should be received with some degree of allowance. In fact the whole subject of morbid growths, benign and malignant, should be revised and re-examined. When there is so much dispute as there confessedly is at present, respecting the real nature of the cancer-cell, or whether indeed there is such a cell at all, it well becomes the practitioner to look with distrust upon many of the alleged discoveries of the microscope. Besides, he should not lose sight of the value of his unassisted senses, nor cease to cultivate them in the highest possible degree."—p. 325, vol. i.

Whether this rather contradictory caveat amounts to a disparagement, we leave the advocates of the microscopic tests to judge. The two lessons to be learned from it, as to the importance of studying closely the ordinary unmagnified external and internal appearances, and in regard to the revision and re-examination "of the whole subject of morbid growths," will not be lost, we trust, upon the reader, however much they may lessen his faith in the present teaching of the author. The whole subject, certainly, might receive more light from existing authorities on both sides of the Atlantic than appears to be vouchsafed to it in the sweeping and rather confident, although really discouraging remarks just quoted.

There are some singular ideas expressed in the next chapter (on scrofula) in regard to the nature of "tubercular matter." This he has "investigated too often, and under too many circumstances, not to be convinced that it is susceptible of organization, although certainly in a less degree than scirrhus and encephaloid." The reasons for adhering to this exploded notion are given at length, but need not be quoted here, as they are well known to the readers of other works of Dr. Gross.

The chapter (IX.) on wounds, and that (X.) on the effects of injuries upon the system, may be studied with profit by every surgeon; while they

are minute and sound enough in precept and practice, to serve as efficient guides to the most unsophisticated student. They are eminently practical and philosophical; which is the highest praise that we can award to any portion of the book.

The chapter (XI.) on syphilis presents a good summary of the facts, doctrines, and principles of treatment of this malady, in which, as usual, he expresses decided views in regard to many mooted points, and gives much excellent advice. An instructive chapter on general diagnosis follows, containing much useful matter in the course of 130 pages, in which is included a good, although brief, account of the microscope and its employment in the study of surgical disease.

The chapter on minor surgery, which comes next, does not strike us as full enough, especially in the section on bandaging. This is very meagre, and, although more susceptible of explanation by diagrams, than most subjects, it is scantily illustrated by three veteran figures, the first of which is badly chosen, and the third is an ancient exaggeration, which has done service as a scarecrow in so many places that it has almost ceased to afford the necessary warning.

In this chapter the merit of introducing the employment of metallic sutures, as substitutes for those of vegetable or animal fibre, is justly awarded to Dr. Sims. We regret, however, that the experience of the author with them is so limited, and that instead of yielding to the influence of prejudice he did not satisfy himself more fully on the subject. We do not believe in the peculiar virtue of silver wire, but we have no doubt of the superiority for suture purposes, of lead and annealed iron wire, as well as the more expensive gold and silver, over the silk or flax.

The five chapters, on operative surgery, plastic surgery, subcutaneous surgery, amputations, excisions of the bones and joints, and anæsthetics, which complete part first, are, generally, well brought up, and contain many valuable practical hints. That on anæsthetics is a good one, in which due honour is done to the claim of the unfortunate Wells, who was undoubtedly forestalled by his more alert pupil, Morton, in winning the first fruits of the renowned letheon discovery. Dr. Gross prefers chloroform to ether, and quotes his successful experience of ten years' duration, as well as that of the European hospitals, in its favour. He is less happy in his employment of freezing mixtures, and does not appear to have tried them in the cases of whitlow, carbuncle, and other more accessible diseases to which they are so much more applicable than to those of the mammary gland.

Part Second commences with a generally judicious chapter on erysipelas, furuncle, anthrax, bed-sores, burns, frost-bites, morbid growths, and other diseases and injuries of the skin and cellulo-adipose tissue, which, although not particularly full, appears to be sufficiently so for ordinary practical purposes.

Passing over the chapter (II.) on diseases and injuries of the muscles, tendons, burses, and aponeuroses, we stop only to remark that we know of no such parts as "burses," and that the plural terminations of the words bursa and aponeurosis cannot be made the same without an error, which is only confusing to the student who may not be aware of our author's peculiarity on the subject.

The next chapter (V.), on diseases and injuries of the arteries, is free from this infirmity, and may be regarded as one of the best of the whole

work. It is admirably illustrated, and so rich in most respects that it is well worth the cost of the volume to the distant practitioner. We would gladly dwell on many points which have attracted our attention, were we not obliged to curtail the remainder of this notice, in view of a whole volume yet untouched.

The chapter (VIII.) which succeeds, on diseases and injuries of the joints, is not so satisfactory, except, perhaps, in treating of dislocations; although on this subject also, we should like to have seen more upon the manipulating methods of reduction, as applied to other dislocations, beside those of Reid and Crosby for the thigh and the thumb. Some generalization on this interesting subject might have been dwelt upon with great advantage. The section on tuberculosis of the joints, and especially on hip-joint disease, is prolix and comparatively long on coxalgia, but, in several respects, and particularly as to the treatment, it does not accord with our ideas of what is due to these grave affections.

But we must hasten on to Volume II., which begins with diseases and injuries of the bones and their appendages. These most important subjects receive a tolerably due share of attention, and, in the section devoted to the diseases of this tissue, are beautifully illustrated.

Fractures are systematically and generally well discussed, in accordance with prevalent views and recent experience. A hospital surgeon may notice some want of precision and minuteness of detail in the directions for the management of some of the special fractures; but, in the main, all the advice and assistance, if not somewhat more, is given that the young surgeon would be likely to find in many of the text-books. We would counsel him, however, to trust to none of these too often halting guides, but to provide himself with such monographs upon the subject as the work of Malgaigne, recently translated, those of Astley Cooper, of R. Smith, of Lonsdale, or even the older ones of Boyer and Desault.

In most respects, however, his teaching is fully up with the times. The immovable apparatus, of which he prefers the form of starch bandage, receives a sufficient share of attention; and the use of adhesive strips, both for retaining purposes and for those of extension and counter-extension, is honourably noticed. Dr. Gross gives the credit of this valuable improvement to his former preceptor, Dr. Joseph R. Swift, of Easton, and claims to have been the first to call attention to the subject in his *Treatise on the Diseases of the Bones and Joints*, published in Philadelphia so long ago as 1830. We believe that Dr. Crosby, of New Hampshire, is another claimant for priority in this matter. Like many other good things in practice, it appears to have remained unnoticed in the hands of a very few practitioners, until Dr. E. Wallace secured its adoption in the wards of the Pennsylvania Hospital, some fifteen or sixteen years ago, by contriving the extending band, which is now the established favourite in the management of oblique fractures of the femur. The employment of the adhesive plaster for similar purposes in other fractures, and for counter-extension, gradually took place as a natural consequence; but we suspect that the earliest general attention to the value of the expedient for extending purposes is chiefly due to the works on *Minor Surgery* of Dr. Sargent and Dr. H. H. Smith, the former of whom was the first to recommend it as introduced by Dr. Wallace.

Dr. Gross discards the use of the term compound fracture, and treats of fractures under the two heads of simple and complicated only, preferring

to regard what everybody understands by compound fracture as one of the forms of complication merely. We do not see that anything is gained by this innovation, which is not counterbalanced by the confusion inevitably created by the arbitrary substitution of a new and not more explicit adjective for a long established old one. It matters little, except under conventional rules which custom only can control, whether we call fractures with wounds extending to the surface, compound or complicated, if, in describing them as either, we have to state the nature and extent of the special complication.

Some wise cautions are given as to the application of the bandage in the treatment of fractures. The danger is so great of undue or irregular compression of the limb, and of injurious disturbance of the fragments, especially by awkward hands, in the employment of this part of the dressing, as recommended by our author, that we are surprised at his urging it, in all fractures, under the retaining apparatus. We are satisfied that, except in occasional cases, it is better practice to omit the under bandage altogether. This was the conclusion long ago arrived at in our part of the country, as the result of a very large hospital experience among dressers of undoubted skill and judgment; and for the same reasons will prove, with easily recognized exceptions, much the safer policy for less expert practitioners in private practice. We believe that even the many tailed bandage of Scultetus, as too often applied, is rarely more than an unnecessary aggravation of the patient's sufferings, and may thus become a snare to his attendant.

Dr. Gross prefers, in the treatment of transverse fracture of the patella, a tin case like the old gutter of Boyer, which is a needless complication. Nothing answers better than the simple well padded straight wooden splint of Dorsey, such as could be prepared at once in any private house, with notches or transverse cleets for the attachment of the oblique bandages, or adhesive strips; and these should pass, not around the joint in the old way, but along the sides of the limb only, so as to avoid the injurious constriction of the circular strips and of the figure of 8 bandage.

A cut and description are given of what is called the "mode of dressing pursued by Professor Hamilton, and also with slight modifications, by Dr. Neill and his colleagues, at the Pennsylvania Hospital." We do not think that the learner will be able to profit much by the explanation given, or that Dr. Neill and his colleagues will agree to be quoted as pursuing, with slight modification, the plan represented. Dr. Neill's method, as we understand it, requires the application of the strips directly to the skin above and below the separated fragments, without the intervention of a compress which is only in the way, and attaches these strips obliquely to the margin and under surface of the splint above and below the joint, so as to draw upon the fragments in opposite directions, holding the lower fragment in its proper place and drawing down the upper into apposition with the lower. In this method the pressure is applied only to the fragments and immediately adjacent soft parts, care being taken to obviate the tilting of the fragments which may otherwise prevent the approximation of the fractured surfaces; and the whole is left as much uncovered, during the early part of the treatment, as the retaining bands will allow. The mode of using the strips under the bandage, in his plan, is not clearly described by Dr. Gross, but would seem to be a good one; the upper fragment being apparently drawn down and retained by a series of long strips passing diagonally along the sides of the joint, and then retained by vertical and transverse

strips encircling the limb below the joint. An outline sketch of the ingenious and effective dressing of Prof. Crosby, in which the Spanish windlass principle is adjusted to an arrangement of longitudinal strips, would, we think, have been more useful and made a better show in the margin than the rather clumsy looking picture which he has chosen as the type of the class. In this the strained position of the foot would be intolerable to the patient, and might mislead the student into the idea that there were some extensor muscles to be relaxed in front of the leg as well as of the thigh.

We have dwelt rather long upon these small matters in connection with one very troublesome form of fracture, more because we are thus enabled to give some idea of what we miss, in this chapter, of the attention to minor details, which is so desirable to inexperienced practitioners. The treatment of fractures is made up of little things; and it is in the knowledge of these that the accomplished surgeon will ever show a superiority which all can more or less appreciate in its immediate result, even more clearly than in the final one. It must be especially tantalizing, for instance, to the anxious inquirer for assistance in arranging a dressing of adhesive strips for fractured clavicle, to find the only answer given in the remark that "when this dressing, which is more easily applied than described, is carefully put on, it answers the object much better than any of the numerous contrivances that have ever been invented for the cure of this fracture." He might just as well have said, what is equally true and so much more consoling, that rest in position on the back, without other adjuvant than a well-stuffed level pillow under the whole chest, answers better still.

The section on fractures terminates with a very interesting and carefully prepared account, under all the usual heads, of fracture of the femur; in which, perhaps, the least satisfactory part is that which relates to the treatment. In speaking of fracture beds, we regret that he did not give us a view of a serviceable fracture bed, such as the ingenious forms alluded to of Coates or Hewson, or, what is more generally available, the simple frame and perforated mattress and sacking-bottom, instead of the obsolete and cumbrous machine of Jenks. We regret, too, that, in addition to a contrivance of his own, which has some unnecessary complications, he did not exhibit the simpler apparatus of Desault and Physick, which, notwithstanding his unfortunate experience in managing it, has done so well in hundreds of cases for half a century. His crutch-headed fracture box is not equal to the long and short splints with cross-piece, foot-board and perineal cushion, of Hartshorne's apparatus, of which it is a modification, or to the apparatus of Flagg, another modification, which has long been used in Boston and New York. Nor is it equal to the admirably simple long splint, with cross-piece and perineal adhesive strips, of Gilbert, or to the splint of Liston modified by Haynes Walton. In no form of fracture is that "simplicity which is the measure of perfection," more attainable or desirable, in the mechanical treatment, than in this of fractured femur, whether in the shaft or about the joints. The very best materials for the purpose are the strong linen sheets, the unbleached muslin bandages, the rolls of carded cotton, the cushions of bran, the pillows, and the splints of wood, bark, binder's board, or sole leather, with the shoemaker's wax, if not sticking plaster, which are within everybody's reach in town or country; and this is the machinery which the student should be taught by diagrams and explanations, however copious, to mould and shape and combine for his own purposes in every emergency, and for all kinds of fractures, without the aid of tinman or joiner.

It is unnecessary to devote more space to the examination of the remainder of the second volume. We may say, in general, that the various topics are mostly well treated, some of them admirably so; and that a few of the sections fall short of the author's aim in providing all that could be desired in "a reliable guide."

Viewing the production, as a whole, we do not consider it by any means the "compilation," which some readers may expect to find, in a comprehensive treatise on an important branch of the healing art. The numerous cases in illustration of theory and practice, the frequent allusions to decided personal experience, the whole tone and spirit of the work, in short, forbid the idea of mere book-making, however much it may be suggested by certain chapters. Indeed, so far as the history, and especially the recent American history, of improvement in surgery is concerned, we should have preferred somewhat more of faithful compilation than has actually been given. We think that the author's countrymen might have been more accurately represented, in several instances, and that in his references to European progress, he is not always free from error.

A few words, before we close, in relation to the purely literary character of the work. Some peculiarities of style have already been alluded to as occurring to an unfortunate extent, in spite of much really good writing and some general excellencies. We should be glad to avoid all comment upon these matters, if we could be allowed to regard an *ex-cathedra* textbook of the classical pretensions of this one of Professor Gross as amenable to criticism only in its scientific aspect.

The importance of a certain measure of purity, if not elegance of style, and of correctness and perspicuity of diction, in a didactic work which is destined to be more or less resorted to by thousands of readers, is too obvious to be insisted on. Terseness, too, and simplicity, no less than freedom from rhetorical flourish, are indispensable characteristics of a truly scientific treatise. We greatly regret, therefore, to have to notice in the language and phraseology of Dr. Gross, frequent evidence of hasty composition, in looseness of expression and confusion of terms, as well as a verbal pedantry, which seriously impair the claim of the work to be regarded as a representative one abroad or a model one at home.

Nor do we think that any force is added to the opposition of the author to instruments, apparatus, or modes of treatment, by the bestowal on them of such epithets, whether just or not, as "villainous," "abominable," "filthy," which are met with in his pages. Another fault, is the affectation of such barbarisms as "fistule," "burse," "polyp," and some other absurdities of word-coining, for which there is no excuse under any circumstances, and in the present case a double accountability which renders them especially improper. Such words are not English, and we do not choose to encourage our author in the vain idea that they have become American.

These are blemishes, however, which are easily removed; the work is so superior to its predecessors in matter and extent, as well as in illustrations and style of publication, that we can honestly recommend it as the best book of the kind to be taken home by the young practitioner. Meanwhile our earnest hope is that a new edition may very soon appear in the proper dress which its accomplished author can so readily afford to it, not only for his own sake, but for that of the American profession, of which he is a prominent surgical leader. *

ART. XXI.—*A History of the Discovery of the Circulation of the Blood.* By P. FLOURENS; Perpetual Secretary of the Academy of Sciences (Institute of France); Member of the Royal Societies and Academies of Science of London, Edinburgh, Stockholm, Munich, Turin, Madrid, Brussels, etc. Professor at the Museum of Natural History at Paris. Translated from the French by J. C. REEVE, M. D. Cincinnati: Rickey, Mallory and Company, 1859. 12mo. pp. 178.

IT is well known that all great discoveries are made but slowly. No one, indeed, can study the rise and progress of any of the inductive sciences without being struck with the fact that such discoveries are not sudden additions to the common stock of knowledge made by a single observer, but are, in reality, the slowly matured results of the patient labours of many individuals who have toiled diligently at different epochs of time, perhaps, and in different countries. Years of patient drudgery are required for the discovery of facts. These facts are recorded and oftentimes forgotten, to be rediscovered and again recorded as new. For a long time their true value and relations are not perceived. They are looked upon as isolated details having no necessary connection. From time to time, however, as the details become more and more numerous, attempts are made to classify and reduce them to some definite order. Such attempts gradually prepare the way for the recognition of the great discovery which is to coordinate these details and to stamp them with their exact value. As the fulness of time approaches, and men begin to attach a deeper significance to the slowly accumulating facts, it often happens that different individuals in various parts of the world, are, unknown to each other, working out the same idea, or hopefully pursuing the same line of inquiry. Occasionally they catch rude and uncertain glimpses of the truth which is afterwards to startle the world, but the data for the generalization towards which they are tending not being complete, they never comprehend that truth, their eyes are not blessed with beholding the discovery. At length the discoverer appears; the man who, with comprehensive mind, grasps the details worked out by his predecessors, supplies by his own investigations the deficient data, and rising above the facts, seizes, as if suddenly and by inspiration, the great truth for which the world was waiting. Thus the discoverer stamps with the seal of his own name the labors of all those who preceded him in the particular field of his research.

Such is the history of the discovery of the law of gravitation; such the history of the discovery of the cell doctrine—the fundamental theory of statical physiology; and such the history of the discovery of the circulation of the blood—a discovery which marks a new era in the science of physiology, and therefore in that of medicine. This discovery was not a sudden performance effected without preparation by a solitary observer, in a happy moment of inspiration, but the gradual, and we may say, anticipated result of the patient and long-continued observations, the judicious experiments and careful generalizations of many thinkers and observers.

The inception of a great discovery is generally preceded by the exposure and overthrow of prevalent and more or less firmly established errors. This was especially the case with the discovery of the circulation. According to

our author, three principal errors masked the important fact of the circulation of the blood. The first was that the arteries contained air only; the second, that the septum between the two ventricles of the heart was perforated; and the third, that the veins carried the blood to the extremities instead of bringing it from them to the heart.

A moment's reflection will show that the discovery of the circulation was an impossibility so long as such erroneous notions were maintained by anatomists and physiologists. To Galen belongs the lasting honor of demonstrating that the arteries contain blood and not air. Vesalius, the father of modern anatomy, overthrew the notion of a perforated ventricular septum. Servetus showed that the veins carried blood to the heart, and not away from that organ. These three illustrious observers are justly to be regarded, therefore, as the pioneers of the important discovery under consideration.

Erasistratus maintained that the arteries are filled with air. He taught that air entered the lungs by the trachea; from the trachea it passed into the pulmonary vein, thence into the left ventricle and the arteries, and by the latter was distributed to the system. As long as the teachings of this ancient philosopher were implicitly accredited, as long as one part of the vascular apparatus was regarded as a channel for air, and another part a channel for blood, the conception of the true idea of the circulation was an impossibility.

Galen observing that blood always escapes, when an artery is opened, concluded that the blood was either contained in the artery, or flowed into it from some other part of the body. He thought that the latter supposition could not be true, for if the artery contained nothing but air, the air should escape before the blood. This he saw was not the case. He noticed that blood alone issued from a punctured artery. He ligated an artery in two places, and opening the vessel between the ligatures, found nothing but blood therein. Thus, by observation, experiment, and reflection of the simplest character, Galen overturned the first of the three ancient hypotheses which for so long a time had interfered with the discovery of the circulation.

"Thus then," writes Flourens, "it was established that the arteries do not contain air; they contain only blood like the veins; an entire half of the sanguineous system, which had been detached by an hypothesis, was given back to it again; and, as the circulation is but the unceasing movement of the blood from the heart into the arteries, and from the arteries into the veins, and through the veins back again to the heart, so long as the arteries were supposed to contain nothing but air the discovery of the circulation was impossible; without the first step which Galen made, it was impossible to make a second.

"Of the three principal errors, then, first mentioned, there was one less; one was destroyed. But Galen was not so happy with the two others. He still believed that the septum between the two ventricles was perforated, and that the veins carried the blood to the extremities; two errors which were destined to pass from him to the moderns, and the latter of which is opposed to the very idea of the circulation."

As the result of his own observations and experiments, Galen believed and taught that there were two kinds of blood—the *spiritual* or arterial blood contained in the arteries and left ventricle, and the *venous* blood filling the veins and right side of the heart. The spiritual blood, he thought, nourished organs of light and delicate texture, such as the lungs; the venous blood nourished those heavy and gross, such as the liver. The nutritive

power of the venous blood was derived from the *spirit* mixed with it, and this spirit, the purest part of the blood, was generated, according to Galen, in the left ventricle. Entertaining such views, and believing, moreover, that in both the veins and arteries the stream of blood flowed away from the heart to the extremities, Galen was forced very naturally to adopt the erroneous notion of a perforated ventricular septum. For by this hypothesis only could he account for the mixture of the *spirit* contained in the left ventricle, with the venous blood filling the right. Galen, therefore, held the septum to be perforated because he had imagined a system which rendered a communication between the ventricles necessary. Mondini, Vasseus or Le Vasseur, and many others of the early modern anatomists, blindly adopted the opinion of Galen. At length, in 1521, Berenger de Carpi doubted, and, in 1555, Vesalius finally denied, positively, the existence of interventricular openings, and thus removed the second great error which opposed the discovery of the circulation. At this juncture appeared Michael Servetus, the discoverer of the pulmonary or lesser circulation, and the victim of Calvanistic bigotry and fury. At his advent it was generally known that the arteries as well as the veins are filled with blood, that these two sets of vessels contained different kinds of blood, and that the partition between the ventricles was not perforated. All this was essentially preparatory to the discovery of the pulmonary circulation. It was still believed, however, that the veins, like the arteries, conveyed blood from the heart. This remaining error had yet to be overthrown. For its overthrow we are indebted to Servetus, who, though at first educated as a physician, afterwards became a divine, and wrote a work entitled *The Restitution of Christianity*, a most remarkable compound of theology, metaphysics, and physiology. A copy of this extraordinary volume, the only copy perhaps in existence, is contained in the library of the French Institute. Curiously enough it is the very copy which once belonged to Colladon, one of the accusers raised up by the pitiless Calvin against the unfortunate Servetus. The passages upon which are based the accusations against Servetus are underscored in this copy by Colladon himself. A frightful souvenir, as Flourens calls it, of this unlucky volume is the scorched and blackened appearance exhibited by several of the pages. It appears to have been saved from the pile where author and work were burnt together, only after the conflagration had commenced.[1] In this theologico-physiological treatise occurs that remarkable paragraph concerning the pulmonary circulation

[1] A reprint of the *Christianismi Restitutio* appeared in 1790, in which the first edition is reproduced as far as the 734th page. A copy of this work was bequeathed by a Dr. Sims to Dr. George Sigmond, author of a work entitled *Unnoticed Theories of Servetus*. To that copy the following note was prefixed: "The fate of this work has been not a little singular. All the copies except one were burned along with their author, by the implacable Calvin. This copy was secreted and saved by D. Colladon, one of the judges. After passing through the library of the Landgrave of Hesse Cassel it came into the hands of the celebrated Dr. Mead, of London, who endeavoured to publish a quarto edition of it; but before this edition was nearly completed, it was seized on the 27th of May, 1723, at the instance of Dr. Gibson, Bishop of London, and burned—a very few copies excepted. The late Duke de Valliere gave near 400 guineas for this copy; and at his sale it brought 3810 livres." According to Flourens, Mead gave it to Boza, and subsequently it was purchased for the Royal Library at Paris, for a very high price. (See an interesting brochure entitled, *Calvin and Servetus: the Reformers' share in the Trial of Michael Servetus Historically Ascertained*, translated from the French of A. Rilliet, by the Rev. W. K. Tweedie, Edinburgh and London, 1846, p. 68.)

which has given rise to so much discussion, and which, more than anything else, more than his sad fate even, has preserved his name from oblivion.

"This communication," writes Servetus (*i. e.* the passage of the blood from the right to the left ventricle), "takes place not by the middle septum of the heart, as is generally believed, but by a remarkable artifice; the subtle blood of the right side of the heart is agitated in a lengthened course through the lungs, whereby it is elaborated, from which it is thrown of a crimson colour, and from the vena arteriosa (pulmonary artery) is transfused into the arteria venosa (pulmonary veins); it is then mixed in the arteria venosa itself with the inspired air, and by the act of expiration is purified from fuliginous vapors, when, having become the fit recipient of the vital spirit, it is at length attracted by the diastole."[1]

From passages in the writings of Galen[2] and Vesalius,[3] we may conclude that both these illustrious men had some vague idea of the fact that the blood of the right ventricle passed, at least in part, through the pulmonary artery into the lungs. But, as Flourens very justly observes—

"This was only the half of the truth. The entire and complete idea necessary to establish the pulmonary circulation was to understand that the blood passed from the *pulmonary artery* into the *pulmonary veins;* that the blood leaving the right heart by the pulmonary artery, returned to the left heart by the pulmonary veins; that the blood left the heart and returned to the heart; that there was, consequently, a *circuit, circulation;* and this idea of the circulation, so grand and so new, was first formed by Servetus."

"Now," continues Servetus, "that the communication and preparation take place as stated through the lungs, is proclaimed by the various conjunctions and communications of the arterial vein with the venous artery. The remarkable size of the arterial vein (pulmonary artery) confirms this, a vessel which could neither have its actual constitution nor dimensions, nor transmit such a quantity of the purest blood direct from the heart itself for the mere nourishment of the lungs. Neither would the heart supply the lungs in such proportion (especially when we see the lungs in the embryo nourished from another source), by reason of those membranes or valves which remain unopened until the hour of birth, as Galen teaches. The blood, consequently from the moment of birth, is sent, and in such quantity is sent, for another purpose from the heart into the lungs; from the lungs also it is not simple air that is sent to the heart, but air mixed with blood is transmitted through the arteria venosa (pulmonary vein). In the lungs consequently does the mixture take place. The crimson colour is imparted to the spirituous blood by the lungs, not by the heart. There is not room enough in the left ventricle of the heart for so important and so great an admixture; neither is there space there for the elaboration into the crimson colour. Finally, the septum medium, seeing that it is without vessels and properties, is not adapted to accomplish that communication and elaboration, although something may transude through it."

"All this," observes Flourens, "is full of sagacity, acuteness, and penetration. The connection or union of the pulmonary artery with the pulmonary vein in the lungs by an infinite number of branches; the calibre of the pulmonary artery, which would be much too large if it served only for the nutrition of the lungs; the nutrition of this organ in the embryo without the blood of the pulmonary artery, which, indeed, does not then transmit any; all these are most excellent and decisive reasons—the same which we now give—the true ones."

To Servetus belongs another honour. He not only discovered the true route of the blood from one side of the heart to the other through the

[1] Christianismi Restitutio. Viennæ Allobrogum, 1553.
[2] De Hippocratis et Platonis decret., vi. 264.
[3] Opera Omnia Anat., 1725, i. 517.

lungs, but he also discovered the true place of *sanguification*, of the *transformation* of the blood, of its change from black blood to red. Galen placed the seat of sanguification in the liver; Servetus first located it in the lungs; a truth which was not then remarked, which was not comprehended until long afterwards, and which, indeed, did not receive its full development until very recent times by the experiments of Goodwyn and Bichat.

"The passage of the blood from one ventricle into the other," continues Servetus, "is not through the septum. In the same manner that the blood of the *vena porta* passes into the *vena cava* through the liver, so the blood of the arterial vein passes into the venous artery through the lungs." A nearer approach to the truth could not have been made without finding it. Finally, said Servetus, in closing, and he could safely say it, "if any one will compare these things with what Galen has written in the sixth and seventh books of his work, *De Usu Partium*, he will see clearly the truth which Galen did not perceive."

Six years later, Realdo Columbus, whom Flourens regards as one of the best of the early Paduan anatomists, discovered again and independently the pulmonary circulation. He appears to have been unacquainted with the labours of Servetus, yet we find in his work, *De Re Anatomica*, published in 1559, the following remarkable passage:—

"Between the two ventricles is the septum through which it is believed the blood passes from the right to the left; but this is a great mistake, for the blood is carried by the arterial vein into the lungs, thence it passes with the air by the venous artery into the left ventricle of the heart."

He reasons upon the valves of the heart in very much the same manner as did Harvey, nearly seventy years afterwards. The blood, he says, when it has once entered the right ventricle from the vena cava, can in no way get back again; for the tricuspid valves are so placed that, whilst they give a ready passage to the stream inwards, they effectually oppose its return. The blood continues to advance from the right ventricle into the vena arteriosa or pulmonary artery; once there it cannot flow back upon the ventricle, for it is opposed by the sigmoid valves situate at the root of the vessel. The blood, therefore, agitated and mixed with the air in the lungs, and, having thus in some sort acquired the nature of spirit, is carried by the arteria venosa or pulmonary vein into the left ventricle, whence, being received into the aorta, it is, by the ramifications of this vessel, transmitted to all parts of the body.

Notwithstanding the accuracy of the above language concerning the course of the blood, we have only to turn to other portions of his writings to learn that Columbus could never have entertained the true Harveian notion of the circulation. He denies the muscular character of the heart, and thinks, with Galen, that all the veins of the body originate in the liver; that the portal vein arises by innumerable roots from the concavity of the liver; and that the vena cava and its branches originate from the convexity of this organ, and carry the blood that is requisite to nourish and maintain every part of the body.

Next in the chronological order comes Andreas Cæsalpinus, of Arezzo, who, without referring to Columbus, also describes the pulmonary or lesser circulation in the subjoined words:—

"This circulation which carries the blood from the right heart through the lung into the left corresponds perfectly with the disposition of the parts. For each ventricle has two vessels, one by which the blood arrives, and the other by

which it departs. The vessel by which the blood arrives at the right ventricle is the vena cava, that by which it leaves is the pulmonary artery. The vessels which pour the blood into the left ventricle are the pulmonary veins, the vessel which affords its exit is the aorta."[1]

Cæsalpinus appears to have been the first to use the word *circulation;* and the language which he employs in several parts of his work, indicates some knowledge of the general or systemic, as well as the lesser or pulmonic circulation.

"He was the first," according to Flourens, "and the only one before Harvey, who called attention to the swelling of the veins which takes place *below* and never *above* the ligature. It is a very curious thing, he observes, that the veins become distended below the ligature and not above it. Those who bleed patients, he adds, are familiar with the fact; they always place the ligature *above* the place of puncture and not *below* it: *quaia tument venæ ultrâ vinculum non citrâ;* which should be just the contrary if the movement of the blood was from the heart towards the external parts of the body."

In another place he says:—

"The blood conducted to the heart by the veins, receives there its perfection, and this perfection acquired, it is carried by the arteries to all parts of the body."

Flourens observes that a better conception of the general circulation could not be found, nor a better definition be given in as short a sentence.

We are constrained to differ from the critical estimate which M. Flourens is disposed to place upon the views of Cæsalpinus concerning the circulation. This celebrated peripatetic philosopher appears to have had no conception of the heart as the efficient cause of any motion which the blood might have; he was ignorant of the existence of the valves of the veins; in different parts of his work he still speaks of the permeation of the septum ventriculorum by the blood; and he says that the swelling which takes place in the veins of an extremity beyond a ligature is due to the effort made by the blood to get back to the focus or centre, lest, through the compression of the veins, it should be cut off and suffocated. He speaks, it is true, of the intercommunication of the minute arteries and veins, but adds that it is only during sleep that the blood passes from the former into the latter class of vessels; for it is during sleep, he thinks, that the veins become distended, whilst the pulsations of the arteries are then moderated. As has been observed by Dr. Willis, the translator of Harvey's works,

"He plainly sees no connection between a delivery by the artery and a filling by the vein. It is along with all this, and as if to settle the question of the kind of knowledge Cæsalpinus had of the movement of the blood, that he uses the old word Euripus, to express his idea of its alternating or tide-like motion."

Entertaining such notions, it is evident that his conception of the circulation as a whole must have been very confused and unsatisfactory.

Cæsalpinus, however, was undoubtedly a man of broad and comprehensive views. He was in advance of his contemporaries not only in his ideas concerning the circulation, but also in his views of natural classification. He compared and classified plants, not according to their external characters, as was then the custom of botanists, but with reference to their organs of fructification, their flowers, fruits, and grains. In so doing he anticipated, to a certain extent, Cuvier's great discovery of those fundamental laws of

[1] Quæstionum Peripateticarum. Venise, 1593, v. 125.

comparative anatomy, the correlation or harmonization of organs, and the subordination of parts. It was in 1583, in his work, *De Plantis*, that Cæsalpinus announced his new method of classification based upon organism. More than two hundred years later Cuvier carried this methodological principle into the Règne Animal, and used it in the most masterly manner in his unwearied attempts at the exact classification of animals. We are indebted to Flourens for an admirable "*History of Cuvier and his Scientific Labours.*" In that work he dwells with enthusiasm upon the extensive and philosophical use which Cuvier made of the above mentioned laws of organization. He speaks of the application of these laws by Bernard and Jussieu in the vegetable kingdom, but, unaccountably enough, makes no allusion to the early and sagacious recognition of these laws by Cæsalpinus.

The next step towards the establishment of a full and complete knowledge of the circulation, was the discovery of the valves of the veins, made in 1574, by Fabricius ab Acquapendente.

"Fabricius saw well that the valves open towards the heart. They oppose, therefore, any passage of the blood from the heart to the external parts in the *veins;* it must go then from the parts towards the heart—the reverse of what takes place in the arteries, which have no valves. The valves of the veins are the anatomical proof of the circulation—the proof that it makes a circuit, that it returns upon itself, that it circulates. But Fabricius did not understand this proof; he saw the fact, but failed to draw from it that important deduction which was left for the genius of Harvey. He believed that the only use of the valves was to prevent too great an accumulation of blood in the inferior parts of the body, an occurrence which would be attended with the double inconvenience of too great a supply to the lower parts, and too small a quantity in the upper."

Gassendi, in his *Vita Peyreschii*, says that the discovery of the valves of the veins was really made by an Italian writer named Sarpi, who confided it to Fabricius. But of this statement, Flourens concludes with Tiraboschi, the historian of Italian literature, more and other proof must be furnished, before it can be considered established.

M. Portal, in his *Histoire de l'Anatomie*, says that Vasseus or Le Vasseur, a disciple of Jacques Sylvius, and the author of a small Latin abridgment of the anatomy and physiology of Galen, "knew almost as much as we do of the circulation of the blood." But a passage which Flourens quotes from his writings is sufficient to show that he knew nothing of the circulation, not even of that which goes on through the lungs.

Passing over the claims, therefore, of Sarpi, Le Vasseur, and others, we come at length to the illustrious Harvey.

"When Harvey appeared," writes our author, "everything relative to the circulation of the blood had been indicated or suspected; nothing had been established; and this is so true, that Fabricius, who comes after Cæsalpinus, and who discovered the valves of the veins, knew nothing of the circulation. Cæsalpinus himself, who so plainly perceived the two circulations, mixed with the idea of the pulmonary circulation the error of a perforated septum. *Sanguis partim per medium septum, partim per medios pulmones * * * * ex dextro in sinistrum ventriculum cordis transmittitur.* Servetus said nothing of the general circulation. Columbus repeated, after Galen, the fictions of the origin of the veins in the liver, and the transmission of blood to the extremities by them."

[1] De Venarum Ostiolis; Hieron. Fab. ab Acquapendente Opera Omnia Anatomica, 1737, p. 150.

Harvey was thoroughly acquainted with all that had been done before his time for the anatomy and physiology of the circulation. In his *Exercitatio Anatomica de motu cordis et Sanguinis*, he quotes Fabricius and Columbus. He was educated, moreover, at Padua where the state of the question was fully understood, and where everything which had been said upon the circulation was known by all. His work is indeed a masterpiece. Flourens, always positive and always enthusiastic, says, that "this little book of a hundred pages is the most beautiful volume on physiology." Harvey commenced his labours by studying the movements of the heart. He observed in the first place that the contraction of the auricles was not synchronous with, but was followed immediately by the contraction of the ventricles. He saw that the systole of the right auricle had the effect of driving the blood into the right ventricle; that the contraction of the latter forced it into the pulmonary artery which carried it into the lungs and delivered it to the pulmonary veins, whence it was poured into the left auricle. He saw, furthermore, that the contraction of the left auricle drove the blood into the corresponding ventricle; that the systole of the latter caused the blood to pass into the aorta by which it was distributed to all the arteries of the body. From the arteries he suspected that the blood must be transferred to the veins, for it could not well return upon itself. He saw, indeed, that the blood of the two venæ cavæ flowed into the right auricle every time the latter was dilated. He examined the valves of the heart and of the great vessels at its base, and saw from their position and construction that they permitted the blood to flow in one direction only. Then he examined the arteries, and observed that when an artery was opened the blood escaped in unequal jets, alternately stronger and feebler; and that the stronger jets always corresponded with the diastole of the artery and not with the systole. He concluded, therefore, that it was the impulse, the shock of the blood which distended the artery and caused it to beat. For if the artery dilated of its own inherent power it could not expel the blood with the greatest force at the moment of greatest dilatation. In this conclusion he was confirmed by a case of ossification of the femoral artery which fell under his observation. The artery pulsated below the ossified portion. Harvey next directed his attention to the veins. He examined the valves of these vessels, and drew from them the important deduction which had escaped their discoverer, his master Fabricius. He saw that they permitted the blood to flow towards the heart and in no other direction. He argued therefore that the venous blood flowed from the extremities to the heart. Finally he resorted to experiment. His experiments were few, but decisive; and in this, as Flourens justly remarks, is shown his genius.

"When a ligature is tied tightly around an extremity the blood is arrested in the veins alone, because they alone are superficial; when the ligature is tied tightly the blood is also stopped in the arteries which lie deeper. When a vein is tied the swelling takes place *below* the ligature; when an artery is tied it takes place *above*; the blood then flows in directly opposite directions in the arteries and veins; it flows from the extremities to the heart in the latter, and from the heart to the extremities in the former. When any artery whatever is opened, and the blood allowed to flow freely, all the blood of the body is lost through this opening; then all parts of the circulating system communicate with one another, heart, arteries and veins. A moment's reflection, in truth, upon the marvellous rapidity of the movement of the blood will convince one that it must necessarily be thus; for scarcely has the blood arrived at the heart when it leaves it and enters the arteries, no sooner has it entered them than it commences to

pass into the veins, and from the veins it goes immediately into the heart again; this course, this continual *return*, is the circulation."

Such were the observations, such the experiments, and such the logical reasoning with which Harvey worked out the great physiological problem of the circulation of the blood. With him the doctrine of the circulation was not a sensible demonstration, but a profoundly logical inference or induction from various anatomical facts which, when rightly interpreted, showed that the passage of the blood from the arteries into the veins, and from the veins into the arteries, was a necessary movement. Harvey never beheld the passage of the blood from the arteries to the veins. He appears to have had no idea of an intermediate or capillary network. For the demonstration of such a vascular rete the microscope was necessary, and the intelligent application of this instrument, or rather of magnifying powers of the rudest description, to the study of anatomical structure, cannot, it appears, be traced further back than the time of Malpighi and Leuwenhoek. It is a curious fact that Malpighi, the demonstrator of the capillary system, was born in the very year in which Harvey's work on the heart was published. The microscopic researches of the eminent Bolognese on the blood-corpuscles, published in 1686, were among the earliest of those numerous and laborious investigations which led ultimately to the announcement by Schwann of the cell doctrine in 1838.

In the chapter devoted to "Sarpi and the Valves of the Veins," Flourens thus sums up his opinion of Harvey's claims as the discoverer of the circulation:—

"I have already said that the discovery of the circulation of the blood does not belong to any single man. This grand discovery was only made little by little, and part by part; more than twenty anatomists took part in it.

"Harvey demonstrated the circulation of the blood; but he came from Padua, where Fabricius, who had discovered the valves of the veins, was his teacher; in this same University of Padua, where were formed the germs of all Harvey's ideas, Realdo Columbus, who discovered the pulmonary circulation, was but a short time before professor; and Padua is not far from Pisa, where Cæsalpinus, by the light of genius, caught sight of the pulmonary circulation, and by a brighter flash of the same divine fire saw the general circulation.

"In the discovery of the circulation the point of difficulty was to unite the diverse observations successively made, or, so to speak, the different pieces into one whole; the difficulty was to comprehend the phenomena, and the whole of the mechanism united together; and it is because Harvey was the first who clearly and completely comprehended this whole that the glory has remained his."

From this language it will be seen that Flourens follows the beaten track of criticism in simply coupling the name of Harvey with the enunciation of the ultimate fact of the discovery of the circulation. Notwithstanding the preparation that had been made for this discovery, two prevalent errors still stood in the way of its complete development. These errors were rectified by Harvey. When this illustrious man took up the study of the circulation, the heart was regarded as a passive receptacle for the blood, a reservoir in which blood was generated and spirits elaborated. Harvey recognized in the heart an active propelling agent driving the blood out through the pulmonary artery and aorta. This activity he demonstrated by his experiments upon the living animal. The very first chapter in his essay is devoted to the consideration of the movements of the heart, and the title of his treatise, it will be observed, is "*Excitatio de Motu Cordis*

et Sanguinis," not *de Motu Sanguinis et Cordis.* In regarding the heart as an agent by which the circulation of the blood is accomplished, Harvey was decidedly in advance of his contemporaries. He was ahead of them in another particular also. They believed the blood of the veins to be essentially different from that of the arteries. They could not be convinced, as Harvey was, that the two bloods were in reality one and the same, and that the difference in colour was due to changes wrought in the arterial blood as it passed through the body from the arteries into the veins. Even after Harvey had published his views we find Joannes Veslingius,[1] then professor in the University of Padua, and one of the most profound anatomists of his age, addressing two letters to him and urging among other objections to the new doctrine the notable difference between the colour of the arterial and venous blood. To Veslingius it seemed impossible that the dark-coloured fluid found in the veins could be the same as the bright scarlet blood of the arteries.

To the anatomical and physiological facts discovered by his predecessors, Harvey, as the result of his own observations, added a physiology of the cardiac movements so complete and satisfactory in all its details that it remains to this day, like the subsequent experiments of Hales upon the circulation, a model of research. In Harvey were happily combined the two elements of the great discoverer. His perceptive or observational powers were excellent, and he exhibited the generalizing or inductive faculty in its most eminent degree. A good observer, he was still a better generalizer. His mind, indeed, was remarkably assimilative and logical.

"The facts he used were familiarly known, most of them to his predecessors for nearly a century, all of them to his teachers and immediate contemporaries; yet did no one, mastering these facts in their connection and sequence, rising superior to prejudice, groundless hypothesis, and erroneous reasoning, draw the inference that now meets the world as irresistible until the combining mind of Harvey gave it shape and utterance."

M. Flourens' *History of the Discovery of the Circulation* is a most admirably drawn and deeply interesting treatise. The style in which it is written—the *style coupé* of rhetoricians—is remarkable for its clearness and conciseness, and for its spirited and lively effect upon the mind of the reader. The facts are chosen with care and discrimination, and the different steps of the discovery are exhibited in an order at once logical and chronological. The circulation of the adult is discussed in the first chapter. The second is devoted to the fœtal circulation. The third is occupied with an account of the discovery of the lacteals, the receptaculum chyli, and the lymphatics. In the fourth chapter are considered the claims of Sarpi as the discoverer of the valves of the veins. In Chapter V. our author treats of "Servetus and the formation of the spirits." Chapters VI. and VII. contain an account of the contest between ancient and modern physiology, together with some notice of the doings of the "Faculty of Paris," and of the doings and sayings of the eccentric Guy-Patin, the French representative, *par excellence*, of the Greek and Latin epoch in medicine, or, as Flourens calls it, the erudite period of French medicine.

M. Flourens writes *con amore*, and with a degree of enthusiasm only equalled by his philosophical discrimination. He causes facts and their observers, discoveries and their discoverers, to pass before us as in a moving

[1] Observationes Anatomicæ et Epist. Med. ex Schedis Posthumis. Hafniæ, 1664.

panorama. His critical estimate of these, though not invariably correct, is in the main just and to the point. Though we read the French copy of his treatise when it first appeared in 1854, with a lively and absorbing interest, we have read the translation before us with a pleasure in nowise abated by our familiarity with its contents. This translation we have compared with the original text, and find it to be an accurate and reliable rendering of the French. Dr. Reeve has happily and skilfully preserved the spirited and vivacious style of the author. J. A. M.

BIBLIOGRAPHICAL NOTICES.

ART. XXII.—*Transactions of American State Medical Societies.*

1. *Transactions of the Medical Society of the State of Pennsylvania, at its Eleventh Annual Session, held in Philadelphia*, June, 1859. 8vo. pp. 120, 1859.
2. *Medical Communications of the Massachusetts Medical Society.* Vol. ix., No. 5, 1859. Second Series, vol. v., part 5. 8vo. pp. 130, 1859.
3. *Transactions of the Indiana State Medical Society, at its Tenth Annual Session, held in the City of Indianapolis*, May 17, 1859. 8vo. pp. 48, 1859.
4. *Minutes of the Tenth Annual Meeting of the Medical Society of the State of North Carolina*, held at Statesville, May, 1859. 8vo. pp. 28.
5. *Proceedings of the Sixty-seventh Annual Convention of the Connecticut Medical Society*, held at Middletown, May, 1859. 8vo. pp. 108.

1. THE eleventh annual session of the Pennsylvania State Medical Society was opened by an address from the President, Dr. Smith Cunningham, of Beaver County. The subject of the address is "the popular errors which prevail in respect to the duration of utero-gestation in the female of the human race." The theme is an interesting one, as well in reference to its medico-legal relations, as to its bearing upon the peace, often, of families. It is very discreetly and ably handled by Dr. Cunningham. Notwithstanding the average duration of utero-gestation in the human female, may, with great certainty, be fixed at about 275 days—counting from the time of conception—a limited range of some five days either above or beyond this period is usually allowed; in other words, it is admitted that, while pregnancy may terminate at the end of two hundred and seventy days, it may also be protracted to the end of the two hundred and eightieth day. Although this is, certainly, the usual course of things, still, it is well known to accoucheurs that a fully developed infant may be born within the shortest period mentioned above, on the one hand, while, on the other, gestation may be prolonged far beyond the longest. Popular opinion, however, admits of no such wide deviations. According to it, the period during which the child is carried in the womb—that is, the period which elapses between conception and delivery, is nine calendar months or forty weeks—only a very few days' variation short or in excess of this period being allowed, particularly in the case of a first birth, or when the husband happened to have been absent at the time when conception, according to popular theory, should have occurred in order to initiate a gestation of nine months. It will be readily perceived that, under such a theory, the grossest injustice cannot fail to be inflicted, on very many occasions, upon the most honest and virtuous of women.

In respect to those births which occur previously to the termination of forty weeks, the accoucheur can experience but little difficulty in determining, from the condition of the child when born, whether its birth has or has not been premature. Even, however, where no evidence is presented by the infant of deficient development, the numerous well authenticated facts on record, which prove beyond, we think, the possibility of doubt, that utero-gestation may be accomplished, and a perfectly developed child born, at a period short of forty weeks, should lead invariably to a judgment favourable to the mother, in the absence of any positive evidence of misconduct on her part. As to the cases in which pregnancy is reputed to have been protracted beyond forty weeks, there is always, it must be confessed, no little difficulty in verifying the fact of the prolongation of utero-gestation, inasmuch as we have no positive data by which, in any instance, to determine the exact period of conception. We are obliged in these cases to accept the testimony of intelligent and virtuous matrons, sus-

tained as it is, to a limited extent, by the few instances upon record in which immediately after connection with her husband, the female had been so circumstanced as, apparently, to preclude the possibility of her having had sexual intercourse with him or any other male, up to the period of her confinement.

How far utero-gestation may be prolonged beyond the normal term cannot be settled with any degree of certainty. It has been known to be protracted to the 287th, 294th, and even to the 300th day, and upwards. Cases of this kind have been recorded, and the evidence by which the duration of gestation in each of them is established is as satisfactory and conclusive as it is possible for it to be in reference to a subject of the nature and character of the one under consideration. We have a right, therefore, to assume it as an established truth, that the period of gestation in the human female is liable to considerable variations from the usual term of forty weeks; consequently, no female who has always sustained an irreproachable character for chastity and truthfulness can, with justice, be reproached, or branded with infamy simply from the circumstance that, in her case, utero-gestation has been accomplished within a shorter period or has endured for a longer one than the usual average,

Reports are presented from the medical societies of Beaver, Bradford, Carbon, Indiana, Lebanon, Mercer, Montgomery, Perry, Philadelphia, Schuylkill, Susquehanna, and York Counties. Those from Carbon, Indiana, and Susquehanna, are confined to simply a list of their respective officers and members, while of the remainder, eight furnish very concise, superficial and unsatisfactory sketches of the medical history of the several counties from which they emanate. Some excuse for the bald and, generally speaking, uninstructive character of these reports, may, perhaps, be found in the fact that, throughout the State of Pennsylvania there reigned during the year 1858—the period embraced in the reports before us—an almost unexampled condition of healthfulness; an absence, certainly, of any important epidemic malady, with a very limited prevalence of the usual endemic diseases of the different sections of the State; and in such of these as did occur an unusual mildness of character. Even, however, giving to such excuse all the weight it merits, it appears to us that this very year must have presented a field for medical observation from which, had each portion of it been carefully cultivated by the many talented physicians within the State, much valuable fruit, acceptable to the profession generally, here and elsewhere, might have been culled.

In the report from Beaver County Medical Society, its author, Dr. Stanton, notices the fact, that an "unusually large number of cases of phthisis" had recently occurred in and about the village of New Brighton. During the year 1858, especially the early portion of it, several deaths from the disease were reported, exceeding in number those which had occurred for four or five years previously. Dr. S. can assign no cause that could be considered capable of developing consumption in the locality referred to. There are few places in the State that can boast of greater healthfulness. At all seasons of the year there is a pleasant breeze up and down the river, furnishing a supply of fresh and pure air, while, at the same time, the surrounding hills protect it from the cold winds of the winter and spring. Dr. S. is of opinion that it was by contagion the increase in the number of cases of consumption at New Brighton was produced. It may be proper to remark that most of the deaths above referred to, took place in quite aged patients who had been labouring under all the symptoms of consumption for many years.

We are led to believe, from statistical information which we have been at some trouble to collect, that consumption of the lungs is a disease the prevalence of which has not only increased in the village of New Brighton, of Beaver County, but that it is increasing every year among the residents of all the interior villages of the State of Pennsylvania, and among her rural populations generally; and in a ratio approximating very nearly to their respective increase in wealth and luxury. The prevalence of consumption among the families of our villagers and farmers can be shown, we believe, to have kept step with the deviation of these families from their former frugal, active, and industrious manner of life, and their adoption of the absurd practices which characterize the mode of living of our fashionable classes in the larger cities. The question as to the propaga-

tion of consumption by contagion has been mooted by more than one writer on the disease. That the disease can be communicated by the sick to the well is a popular belief in certain portions of continental Europe, and there are many curious facts upon record, and some have fallen under our own notice, which would seem to favour this belief. A careful investigation of all the evidence bearing upon the subject has convinced us, nevertheless, that tuberculosis, whether of the lungs or any other organ, is never due to a contagious emanation from the bodies of those labouring under it.

Dr. S. states that, in a portion of the cases of consumption which fell under his notice, glycerole and syrup of the hypophosphites were employed, and, in some instances, with apparent benefit, but in others with no appreciable results. He has seen more advantage from the use of iodide of potassium, combined with various other remedies.

The report concludes with a brief notice of two cases of obstinate hiccough, one of them successfully treated. The first occurred in a girl, sixteen years of age. A *post-mortem* examination showed that the cause of death had been inflammation of the membranes of the brain around the origin of the pneumogastric nerve, with extensive serous infusion. The second case occurred in an old man. This was, at first, supposed to have arisen from gastric irritation, and was treated accordingly, but with no benefit. Recollecting the nature and seat of the lesion in the former case, a blister was applied "to the back of the head and neck." As soon as vesication ensued, the hiccough ceased, and the patient recovered rapidly.

In the report from Bradford County Medical Society, Dr. Holmes states that, during the year 1858, typhoid fever prevailed in that county almost entirely in high localities—having occurred on the loftiest pinnacle of the Armenia Mountain, and also in the most elevated situations in every direction from the village of Canton. "Wherever," he remarks, "it makes its appearance, as a general thing, it was confined to a few families, and sometimes to only one family in the same vicinity; but, when it made its appearance in a family, it generally went through it. The fatal cases were in the proportion of about *one* to *twelve*."

Dr. Holmes relates also a case of sudden death in the keeper of a tavern, caused by seeing a man labouring under delirium tremens cut his throat in his bar-room. Frictions, galvanism, opening the jugular vein, the warm bath and artificial respiration were resorted to with a view to his restoration, but with no effect.

A very excellent sketch, illustrated by a map, is given of the geological features of Bradford County.

No facts, it is remarked, nor statistics, have as yet been collected, adapted to throw any light upon the connection, if any exist, between the physical and medical geology of the county. There is very little iron found in Bradford, and a considerable amount of limestone. Since 1849, agriculture has become greatly increased and improved within its limits. Dense forests of vast extent have, in consequence, rapidly become cultivated farms.

We learn from the report before us, that dysentery has not prevailed to any extent in the county of Bradford, for many years past; the few cases that are met with seldom terminate fatally. The only remarkable change, it is said, that has occurred in the diseases of the county is, the greatly increased prevalence of periodical fevers and other affections of a malarious origin.

In the report from the Lebanon County Medical Society, Dr. B. F. Schneck states that, from November, 1858, to May, 1859, he had attended a larger number of cases of inflammation of the lungs and air-passages, than he ever before had done within the same number of months.

He treated, he remarks, one hundred and seven cases, within the above period—ninety-eight were of pneumonia and bronchitis, occurring almost exclusively in children, seven of croup, and two of pleurisy. They were, in general, very manageable; this he imputes to his having made use, almost invariably, after venesection, when this was required, of the veratrum viride. From an extensive experience of the curative effects of the veratrum in all inflammatory affections, he regards it as a valuable addition to the materia medica. He warns us, however, that its use requires judgment and caution. At first Dr. S. em-

ployed Norwood's tincture, as procured from the druggists; having, however, obtained a very good sample of the root, he prepared the tincture himself, and found this possessed of greater activity—he was often, indeed, startled by the violent effects it produced when given in the usual doses. From half a drop to a drop, given to an infant of from six to twelve months, controlled a pulse of 130—160, as if by magic. Adults were sometimes violently affected by five or six drops, every six hours. Dr. S. has found that the most alarming symptoms which result from the administration of the veratrum, are readily removed by the application of a sinapism over the stomach, and internal stimulation by means of brandy.

For children, the favourite formula of Dr. S. is, ℞.—Syr. ipecac. *vel* syr. scillæ comp. ʒss; sp. ether. nitrici ʒss; tr. verat. virid. gtt. xv—xxv.—M. The dose being from 15—30 drops, every three hours according to the age of the patient. Dr. S.'s invariable rule is to caution the attendants never to give the syrup as an emetic. Two teaspoonfuls of it were given in mistake within an hour, to an infant ten months old, threatened with suffocation, in the course of an attack of pneumonia. Sudden prostration ensued, and, in the midst of violent retchings, and vomiting, the child died. Although this painful occurrence has given to Dr. S. a lively sense of the danger attendant upon the incautious use of the veratrum viride, he is still convinced that, in judicious and intelligent hands, it is one of our most potent antiphlogistic remedies.

An observation that occurs towards the close of the report in reference to the prevalence of criminal abortion within the county of Lebanon, as a cloak to conceal marital offences, has impressed us with a painful sense of the increase of vice and immorality amid the populations of the rural and agricultural districts of Pennsylvania.

Dr. E. Griswold, of Sharon, states, in the report of the Medical Society of Mercer County, that cases of incontinence of urine had been of frequent occurrence in his practice.

"The urine generally reddened litmus, and, in some cases, deposited urates of various shades of colour, mostly pink, or yellowish, dissolved by heat. In one case, the urine dribbled constantly; in the others, and particularly in children, it was passed involuntarily in bed, and sometimes as often as every half hour during the day. The symptoms of constitutional or other disorder present, were treated according to the indications presented by each individual case."

Dr. G. found Tilden's fluid extract of belladonna, given in a solution of bicarbonate of potass, so much superior to any other remedy, in these cases of incontinence of urine, that he now uniformly prescribes it, varying the dose according to the age and idiosyncrasy of the patient. He generally begins with the administration of about one and a half or two drops of the extract. belladonnæ, in a teaspoonful of saturated solution of bicarbonate of potash, two or three times a day for a child three years old. Several cases, on which the remedy appeared to produce no perceptible effect for the first week or ten days it was employed, were cured by continuing it perseveringly for several weeks.

Dr. Poley, of Norristown, gives us, in the report of the Medical Society of Montgomery County, a general statement of his experience in respect to the treatment of inflammatory diseases by large doses of digitalis, subsequently to his first announcement of the practice, in the report of the Montgomery Medical Society, made at the session of the State Medical Society of 1856. He is still confident as to the propriety and success of the practice referred to. He has found, however, that, in cases of inflammation of the brain occurring in children of a year old, possessed of a robust, plethoric constitution, the ordinary dose required is six, eight, or ten drops, every four hours, until its action is beginning to be manifested. In infants of feebler constitution, three or four drops every four hours will be sufficient.

Dr. P.'s experience with the veratrum viride as a substitute for the digitalis in the treatment of inflammation, has shown him that it is far inferior in efficacy to the latter. He found, that when given in doses sufficiently large and frequent to control vascular action, the veratrum occasions much gastric distress, and is transient and irregular in its effects. The preparation employed by him was Tilden's fluid extract.

In this same report two interesting cases of wounds are related by Drs. Wm. Corson, and L. W. Read, of Norristown.

The first occurred in a lad, seventeen years of age. Whilst manipulating with a pistol, charged with two balls, wrapped in a cotton fabric, it was accidentally discharged. The balls entered the left leg, eight inches above the knee, and remained partially imbedded in the head of the tibia. The accident was attended by very severe and continued suffering. The balls were removed whilst the patient was under the influence of chloroform, the anæsthetic effects of which were continued for many hours, in order to quell the persistent pain. Forty-eight hours after the receipt of the injury, decided symptoms of tetanus supervened. These symptoms, it is believed by the relators of the case, were modified, and in the end entirely arrested by the repeated inhalation of chloroform, as the exigencies of the case seemed to demand, during a period of several days. The patient entirely recovered, and with a perfect use of the knee-joint.

The foregoing case has been reported, mainly, we are told, to direct attention to the fact of the employment of unadulterated chloroform for a long period, and with marked benefit—notwithstanding so protracted a continuance of the anæsthetic has been pronounced, by practitioners of experience and judgment, to be attended with great danger.

The second case referred to was that of a strong quarryman, thirty years of age, who was stabbed at midnight, on the 15th of January, 1859. The wound was inflicted by a shoemaker's knife, which entered an inch below the bifurcation of the left carotid artery; completely severing the latter vessel, and passing downwards for two inches. When seen, half an hour after the injury was inflicted, the patient was pale, pulseless, and, to the bystanders, apparently dead. The hemorrhage from the wound had ceased. Efforts were made to resuscitate him, and at the end of fifteen minutes he gasped feebly. Reaction was faint and sluggish, and the patient was incapable of proper mental co-ordination for several days. The pupil of the right eye was dilated. No pulsation could be detected beyond the wound. The right arm and leg were paralyzed. No bleeding accompanied or followed the reaction. The wound was closed by stitches and adhesive strips, supported by a pledget of lint and roller. The recovery of the patient proceeded regularly on to completion. After the lapse of many weeks, he was submitted to the examination of surgeons from Philadelphia, who recognized in him the peculiar condition of one who had had the carotid artery tied. There was no pulsation above the place of injury—the left side of the face was less full than the right; the right pupil was dilated. On the 1st of June, 1859, he was labouring in the quarry, the same as before he was stabbed. He was, however, less strong, and the paralyzed arm and leg were still somewhat affected, and the right pupil continued still dilated.

Dr. J. Newton Evans describes a case of enormous distension of the bladder from retention of urine, in a male sixty-nine years of age. When first seen by Dr. E. his abdomen was much swollen, and the left leg largely œdematous. He had been for some time under the care of a physician who pronounced his disease to be cancer of the bladder, with general dropsical effusion. There was a constant desire to urinate; at each attempt only a small quantity of urine was passed, amounting in all to about a pint in the twenty-four hours. In consultation with Dr. Hill, at four o'clock in the afternoon, seven and a half pints of urine were drawn off by the catheter; at ten o'clock on the morning of the next day seven pints more; at six o'clock in the afternoon an additional five pints; at noon of the third day two pints more, and in the evening another two pints. At ten o'clock in the morning of the fourth day thirty-four ounces were drawn off, and at six in the evening sixteen ounces more. The entire quantity drawn off within the twenty-six hours being two gallons three and a half pints. The urine decreased in amount from day to day, and at the end of eight or nine days it became normal in quantity. The retention in this case was caused by an enlarged prostate. The patient at the end of five months had regained his usual health, and became quite active, riding about daily, although a man of nearly seventy years of age. The necessity of drawing off the urine daily by the catheter continued, an operation the patient was able to perform readily himself.

Dr. Hiram Corson, in the same report, refers to three cases of St. Vitus's dance

—*chorea*—occurring in girls respectively of five, eight, and thirteen years of age, all of which cases yielded in a few weeks to the use of "macrotyn" (*Actæa racemosa?*). To the oldest of the girls a grain and a half, and to the youngest one grain, was given three times daily. Dr. Corson refers also to three cases of incontinence of urine in children—two girls and one boy—aged respectively twelve, fourteen, and eighteen years. In two of the cases the incontinence had continued from early infancy. In the first case a teaspoonful of cubebs, three times a day, cured the patient entirely in less than a month. In the second case the same remedy gave temporary relief; the infirmity returned at the end of eight months, but by a resumption of the medicine the incontinence was again removed. In the third case the cubebs, in large doses and long continued, produced no effect. Twenty-five drops of the tincture of cantharides, given three times a day, stopped the incontinence from the first day it was given. The remedy was continued for two weeks, after which there was no return of the infirmity.

To the report is appended a biographical notice of Dr. James Anderson, who died June 1, 1858, aged seventy-six years.

Dr. Hooke, in the report of the Perry County Medical Society, states that during the year 1858 he saw over ninety cases of enteric fever, eighty-two of which were under his own immediate treatment. About nine out of every ten of these cases terminated favourably. In regard to treatment, Dr. H. remarks that while he has never been in the habit of giving much medicine in this disease, and in a few mild cases has conducted the patient safely through the attack by merely regulating his bowels, still, in the more severe forms of the fever, when there is a full pulse, with pain in the head, and symptoms threatening inflammation of the brain, he never hesitates to bleed, and has had occasion to repeat the operation a second time, and with good results. Dr. H. is convinced that the detraction of a sufficient amount of blood at the onset of the disease, particularly in patients of robust habits, has the effect of rendering the attack milder, and even shorter in duration. He assures us that he has never had occasion to regret a resort to bloodletting in a single instance in enteric fever, where its employment seemed to be demanded. On the contrary, he has had cause, in one or two instances, to censure himself for its neglect at the proper period. He would always, he remarks, rather incur the risk of debility in the after-stages of the disease, from a too free use of the lancet, than the hazard of a fatal inflammation from its omission.

Appended to the report is a biographical notice of Dr. William R. Howe, who died May 24, 1859, in the thirty-third year of his age.

The report of the Philadelphia County Medical Society is made up entirely of biographical notices of Dr. J. K. Mitchell, who died in Philadelphia, April 4, 1858, in the sixty-fifth year of his age; of Dr. J. H. Weir, who died September 12, 1858, in the thirty-ninth year of his age; and of Dr. Gavin Watson, who died on the 28th day of October, 1858, in the sixty-third year of his age.

Passing by the report from the Medical Society of Schuylkill County, which contains an interesting but very rapid and scanty sketch of the principal diseases and accidents observed by the leading practitioners of the county, we come to the report from the Medical Society of York County. With the outlines of a case of diuresis related in it, by Dr. James M. Shearer, we close our notice of the volume before us. The case referred to occurred in a young married woman, aged twenty years, with a child three months old. She had laboured under an excessive discharge of urine for at least eight years. Her thirst was excessive, demanding frequent and large draughts of water. She was emaciated; her skin cold and clammy; her pulse eighty-two, and quite small; her appetite poor; her stools scanty, and devoid of smell. She experienced great pain in the epigastrium, especially after eating. The average quantity of urine passed daily was nineteen pints, of a pale amber colour, and of low specific gravity. Repeated analysis of the urine with Trommer's test for the detection of sugar, proved very clearly that the case was simply one of diuresis. The patient was placed upon a course of tonics; the citrate of iron, with kindred preparations; Dover's powder at night, exercise, friction, and warm bathing, with a proper regulation of diet. After this course of practice had been pursued for about a month, the

general health of the patient is reported as improved; her appetite being much better, and the epigastric pain almost gone, but without any diminution in the amount of urine passed. Dr. Shearer, at the date of the report, considered the prognosis unfavourable.

2. *The Medical Communications of the Massachusetts Medical Society* for the current year, contain only the proceedings of the Councillors, from October 6, 1858, to May 25th, 1859; the proceedings of the Society at its annual session of May 25th, 1859; the Anniversary Discourse, by Dr. Timothy Childs, of Pittsfield, and a brief report of the *zymoses of Massachusetts* during the year 1858—but neither the prize essay, nor either of the professional papers communicated to the Society at its session of the present year have a place in the volume; the omission of these being due, as we are informed by a note from the Committee of Publication, to the low state of the Society's finances.

The subject of the address by Dr. Childs is—Rational Medicine: its past and present—its true relations to specialities, to the partisans of exclusive systems, and to empirics. It is a very plain, unpretending, but at the same time sensible and manly defence of the claims of rational scientific medicine upon its professors and practitioners, and upon the public at large: the debt which it owes to the labours of those who, after having well grounded themselves in the general principles of medical science and practice, devote their time and energies to the investigation of the etiology, pathology, and therapeutics of some particular disease, or of the diseases of a particular organ; and finally, a brief examination of the question as to the proper course to be pursued by medical men and medical associations in reference to the partisans of exclusive systems and special schemes of medicine.

With these latter, Dr. Childs is inclined to observe no middle course. "The moment," he remarks, "you advertise yourself, or allow yourself to be advertised, or written down, or called, a homœopathist, an allopathist, or an anapathist, a hydropathic, botanic, eclectic, electric, analeptic, or *any other sort of doctor*, you cut yourself off, and justly, from the communion of the medical church catholic; and the essence of the quackery is, that you ignore the wisdom and guidance of the past, and assume and advertise yourself to be possessed of a wisdom beyond that of your contemporaries. Here is solid ground. On this ground I would expel a homœopathist, and on the same ground I would expel all other empirics."

Among the "exclusive schemes" of medicine, Dr. C. includes the numerical method as urged by some of its advocates, who would exclude all other methods in the statement, analysis, and appreciation of medical facts, whether etiological, pathological, or therapeutic.

"They tell us," remarks Dr. C., "these exclusives of the school of observations—that 'all true and direct relationships are invariable;' that is to say, that 'a quart of blood, drawn in a given time from the arm, will always, under the same circumstances, produce the same effect; that two grains of calomel, or a quarter of a grain of the tartrate of antimony and potassa, or one half a grain of opium, or these three substances in combination, introduced into the system, every three or six hours, will always, under the same circumstances, be followed by the same results.' They repeat that 'all true and direct relationships are invariable.' Now we venture to say—Not so fast, gentlemen. You must allow us to put some faith in the records of such observers as our science boasts—at least, until the numerical method shall have shown their fallacy; and, in the meantime, do not render worthless your numerical method of statement, analysis, and appreciation, by vitiating its most important element. Do not exact of every poor pleuritic the pound of blood, or the swallowing of just two grains of calomel, or a quarter of a grain of the 'tartrate of antimony and potassa,' or the 'half grain of opium,' or 'these three substances in combination every three or six hours.' We have such tables, and I cannot but see in them an abuse of figures. You are grasping at the shadow of accuracy, and losing the substance, as much as did the mathematicians of the 17th century, with their tables of medicines accurately graduated to the squares of the constitution, and just sixty-eight pounds of bile passing through the hepatic duct in every twenty-four

hours—a conclusion, quaintly observes Dr. Percival, 'repugnant to fact and experience.' The therapeutical results arrived at by the numerical method are mere *averages*, and therefore of no practical value in particular cases. A patient, it has been well said, is never an *average*, but a *case* by and of itself. [*Peaslee's Discourse, New York Academy of Medicine*, 1858.]

"You will observe that it is not the numerical method, but the abuse of it—that is, the *exclusive use* of it—to which I object. Let it be applied wherever and whenever it be found applicable—let us learn all we can from it, but do not confine us to it."

"Every man, observes the late accomplished author of the Philosophy of Medicine, 'in every age, who has stated numerable facts in anatomy, physiology, pathology, or therapeutics, in *specific numbers*, has made use of the numerical method. Every observer who counted accurately his cases of disease, or any of the phenomena connected with these cases, and gave the result *in exact numbers*, instead of resorting to the more common and indefinite terms of 'a small number,' or 'a large number,' 'frequently,' or 'rarely,' has, so far, made use of this method.

"This accurate and beautiful method of observation I do not stand up here to decry. It is its *abuse*, by insisting on it as the *one* and *exclusive* method in the cultivation of medicine, against which I protest. Those ultraists of the rigorous school, date the *dawn* of medical science at about 1827, when Louis, whose name I mention with the profoundest respect, published his great work on the typhoid affection (fever); and the very liberal of them see a dim morning twilight as early as 1808, when Broussais, before he apostatized and set up a theory of his own, published his *History of the Chronic Inflammations*. These gentlemen have no faith in Sydenham or Hunter, or anybody else who has not fulfilled all the requirements of their 'method.' Tell one of them, usually a young man fresh from Paris, that Dr. Nathan Smith found that, with the best treatment he could give to typhoid fever, one in ten dies, he shakes his head; Nathan Smith, though an excellent observer, did not make use of the 'numerical method.' Tell him that M. Louis finds the mortality to be one in nine and seven-elevenths, he takes off his hat."

The report on the subject of zymoses is the first fruits of a system of registration recently introduced by the Massachusetts Medical Society. The object of this system is to institute an inquiry into the occurrence of diseases, especially epidemics, the laws of whose access can only be ascertained by a knowledge of the times, places, and characteristics of their visitations; to collect facts in a more definite form than have hitherto been obtained, in order to elucidate the etiology of a class of diseases by far the most frequent in their occurrence, and the most interesting in their character of any that prevail in this our variable climate.

The simple facts set forth in the report before us are in themselves extremely interesting; but, as is the case in the early working of every measure demanding for its complete and satisfactory execution the cordial approval and personal support of the entire profession, they are at the same time imperfect, and unadapted to lead to any important general conclusions. The committee, however, who have the whole matter in charge, assure us that the success of the system in this the first year of its trial has been as flattering as could have been expected, when the variety of hindrances that present themselves in the way of its accomplishment are taken into consideration.

The system of registration referred to consists in the distribution to each Fellow of the Society of a printed blank, having the names of all the zymoses, with their respective synonyms, in one column, and opposite to these names, for each month of the year, three ruled columns for the reception of the number of cases of the respective diseases falling under the care of the Fellow, according as they were mild, severe, or fatal in their character.

At the close of the year, these individual records are to be returned by the physicians to the secretaries of their respective county or district societies; and those for each county or district being condensed into a single report, are in this form to be transmitted to the secretary of the State Society.

Most of the districts, we are informed, made returns the first year, some of

which were complete, or the result of the records of a majority of the Fellows in active practice. From many districts, however, the returns were from the records of less than a moiety of the practitioners; while from a few districts no return was received.

3. *The Transactions of the Indiana State Medical Society* open with the address of the retiring president, Dr. Talbut Bullard. The theme of Dr. B. is "The Physician, his Trials and Rewards." Truly a fruitful and a noble subject, the simple, truthful narrative of which is pregnant with as much of interest and of grandeur as it is possible for the imagination of the poet or the eloquence of the orator to infuse into any subject submitted to his charge. The physician! the profound scholar, versed in many sciences, familiar with the nature and structure of the human organism, with the laws that govern it in health, and with the changes wrought in that structure, and the disturbance created in the working of those laws by the condition of disease; knowing the causes by which that condition is produced, and the means and agencies by which it may be subverted, and the normal state, and action, and relations of every organ and tissue restored; familiar with the facts and experience, and deductions of past centuries in regard to the science and the art of healing; and while confirming and adding to these facts and that experience, and correcting the errors of those deductions by his own investigations and observations, he presents himself not merely as a professor of the art, and a firm believer in and ardent student of the science of healing, but in a still higher, more important capacity, as the preventer of disease, the conservator of public and of private health. In thus accumulating knowledge, for the benefit, not of himself and family and friends, alone, nor solely of those of his own neighbourhood and nation, but of mankind at large, he is willing to risk life, to resign domestic comfort, to incur the certainty of trials, the nature, extent, frequency, and character of which must be experienced to be appreciated. No description, simply, can portray, no tongue can tell to the uninitiated, the agony, the bitter disappointment, the continued care and anxiety, the fearful sense of responsibility, the heart-sinking which it is the lot of every enlightened and truly sensitive physician to experience, to a greater or less extent, during his career.

When faithful to his calling, as a remuneration for all this labor, and all his trials, the physician, it is true, has his reward, the gratification, the actual recompense which ever follows deeds of charity and pure beneficence; which accompanies the consciousness of the power of doing good, and of the rightful exercise of that power, the power of mitigating human suffering, and alleviating human sorrow, of increasing the sum of man's enjoyment and happiness, of assisting those who are oppressed with sickness and pain, of arresting the hand of death upraised to strike, and, when no more can be done to stay the destroyer, to strip "man's last enemy" of at least a portion of his terrors, by leading him who is "about to perish" to the great spiritual Physician, who is ready to throw across the dark valley a light that shall reveal it as the road to a glorious life which neither pain nor sickness can embitter, nor death again cut short.

The address is succeeded by the report of the Committee on Medical Education, by Dr. Charles Fishback, of Shelbyville. The report is a very candid, outspoken one. It places in strong relief the shortcomings of the systems of medical education at present pursued throughout the United States, and the serious evils which flow from them because of their general inefficiency on the one hand, and, on the other, the facility with which, under their ministration, the ignorant and incompetent, equally with the well instructed and competent, may become invested with the doctorate, and thus legally authorized to take rank with the best instructed and most accomplished physicians of our country.

The measures proposed by the Committee to remedy this state of things, and to inaugurate such a system of medical education as shall insure to the community everywhere the services of learned, skilful, and honest physicians, are legislative action; enlargement and prolongation of the course of study in our medical schools; a fair, strict, and ample examination into the qualification of all applicants for the doctorate; and an effort to enlighten the public mind as to the importance to the community at large of well-instructed medical practitioners.

In reference to these measures, the Society resolved to take immediate action to secure their being carried out, and with the view to enlist in their inauguration and enforcement the countenance and aid of other medical organizations.

The next report is on the treatment of syphilitic diseases without the use of mercury, by Dr. R. E. Haughton, of Richmond, Indiana. It is rather an exposition of the opinions and practice of the leading physicians and surgeons of Europe, in proof that syphilis, in its primary, secondary, and tertiary forms, may be satisfactorily treated and effectually eradicated without the use of mercury, than the presentation of any extended series of observations made by the reporter with the deductions which legitimately flow from them. The report teaches nothing new in evidence of the non-necessity of mercury for the cure of syphilitic affections, nor in respect to the best plan of treating these affections without mercury. With a very slight variation in the language in which it is expressed, we fully concur in the correctness of the sentiment with which the report concludes: "While we have no prejudice against the employment of any remedy when judiciously, appropriately, and timely administered, we must protest against the irrational and uncalled for employment of mercury, not merely in syphilis, but in every other disease."

The following report is on obstetrics. It is by Dr. H. P. Ayres, of Fort Wayne. It embraces the statistics of *ten thousand cases*, furnished to him by practitioners in different parts of the State of Indiana. Among these 10,000 cases there were—

Abortions	288
Miscarriages	242
Twins	178
Stillborn	167
The mother died in labour in	5
Hemorrhage, before, during, or after labour, occurred in	58
Convulsions, during parturition, occurred in	101
Rupture of the uterus occurred in	1
Instrumental aid was resorted to in	68
False presentations occurred in	147
Puerperal fever occurred in	92
Strictly natural labours, without accidents	8653

Dr. Ayres, in commenting on these statistics, remarks, that he has very little confidence in reports of abortions and miscarriages, more especially in private practice, there being so many considerations tending to alter materially their reliability.

In respect to twin cases, the foregoing report presents double the percentage of any of the other reports that have fallen under the notice of Dr. A. Whether this is owing to the natural fecundity of the females of Indiana, or to the climate, he does not pretend to decide.

Deaths occurring during labour, and within the month, are not one-fifth so frequent as in Great Britain, and they will bear a very favourable comparison with the numbers given in any of the reports known to Dr. A.

The cases of hemorrhage before, during, and after labour, largely exceed in number those of other reports—being about, in comparison, three to one. Why it is so he cannot say. The same is true in reference to puerperal convulsions. In Indiana, so far at least as Dr. A.'s information, which is imperfect, extends, there occur six cases to one, in excess of any report that has as yet fallen under his observation. And yet, notwithstanding the excess of hemorrhage and convulsions, there are fewer deaths by about the same ratio.

But a single case of ruptured uterus is reported; in this a post-mortem examination revealed a carcinomatous state of the organ.

Of false presentations there were 147 in the ten thousand cases, which is a very small percentage.

Instrumental labour occurred in only half the proportion of cases compared with other reports.

In respect to puerperal fever, there is an excess over all other reports. For this excess Dr. A. is unable to offer any satisfactory reason.

An interesting case of abscess, succeeding childbirth, is related by Dr. Thos. B. Harvey, of Plainfield, which we shall notice elsewhere.

The report on Fractures and False Joints, by Dr. Meeker, of Laporte, which follows, is a continuation of that presented at the session of 1858. It is mainly occupied by a consideration of the subject of ununited fractures, their causes, and treatment. After a rapid view of the circumstances and conditions that most usually interfere with the prompt and effectual consolidation of fractures occurring in the bones of the extremities, and a brief consideration of the means recommended by the leading surgical authorities to bring about bony union when a false joint has been in consequence created, Dr. Meeker remarks:—

"In summing up the treatment in ununited fractures, I have come to the following conclusions, from my own experience, and from all that I can learn from others, that we should, in the first place, where the case is not of long standing, resort to rubbing the ends of the bones together, with firm dressings, as pasteboard splints and a starch bandage, or leather splints moulded to the part, so as to make pressure, until a sufficient increase of action is manifested in the part, which will be known by pain and swelling. If we do not succeed in producing this result in from one to two weeks, I would then apply the splint recommended by Mr. Smith, and allow the patient to get up from bed; if in a lower extremity, allow him the use of crutches, giving some motion to the fractured part until pain and sensibility were manifest; then apply pretty firm pressure, and continue this treatment, if necessary, for six months. If this failed to produce the desired result, I would then make an incision down upon the fractured bone, saw off the ends of the bone, or cut them off with a pair of bone forceps, bring the ends of the bones together, and apply the same dressings as in recent compound fracture.

"Although much may have been said in disparagement of this operation, I am fully satisfied that in cases of long standing, where there is a false capsule formed around the ends of the bone—they have become rounded and smooth by absorption, or a ligamentous union joins the bones together—then this is the operation which we should perform; and, with the aid of chloroform, we prevent much of the suffering of the patient, and prevent, in a great measure, the shock which the system would otherwise receive from the operation. This would lessen very materially the danger heretofore ascribed to such operations by surgical writers."

Speaking of the treatment of false joints by the introduction of a seton between the united fragments of bone, first proposed and practised by the late Professor Physick, Dr. Meeker very gravely informs us that Dr. P., when he suggested this plan, was "of New York." The doctor ought certainly to have been better informed than to be led into so gross an error. Dr. Physick never practised as a physician or surgeon in New York; from the commencement of his professional career to the day of his death, he resided in his native city, Philadelphia.

A report on the microscope is next in order. It presents, in a kind of general summary, the importance of the facts revealed by the instrument for the formation of an accurate diagnosis of disease. A very correct though general and superficial sketch is presented of some of these facts, of the very existence of which we should be ignorant but for the aid of the microscope. The report is drawn up by Dr. Calvin West, of Hagerstown.

Two cases of amputation of the thigh are reported by Dr. Horace Winton, of North Manchester:—

1st. A man received a severe kick on the shin, which was followed by erysipelas. He was treated during ten days by a Homœopath, with blisters (?) from the knee to the ankle of the affected limb. No improvement resulting, Dr. W. was called in (Feb. 5, 1859). The patient was much emaciated, and greatly debilitated in consequence of the excessive purulent discharge from the diseased limb; his pulse was 120, and very feeble; his tongue dry and black—in a word, he had become sunk into a low typhoid condition. The leg was much swollen, and in places gangrenous. A tonic and alterative treatment, with proper local applications was resorted to, but without any decided benefit. The

diseased limb was then amputated above the knee, after which the patient rapidly recovered.

2d. The second case was that of a man, 25 years of age, of a scrofulous habit. The right knee-joint became inflamed when he was eleven years old, suppuration ensued, with a continual discharge of purulent matter for several years subsequently. The knee finally healed, but the limb was left in a weak condition. In April, 1858, whilst the patient was engaged in moving a building, he received an injury in the same knee, followed by inflammation and suppuration. Two openings were made into the abscess, which gave discharge to half a pint of pus daily. The patient suffered much and became greatly emaciated. Twenty-six days after the abscess was opened, the limb was amputated, several inches above the knee. No union of the flaps took place, in consequence of the unhealthy condition of the parts, and at the end of two weeks a second amputation was performed. Under an appropriate tonic treatment the patient rapidly improved, the stump healed kindly, and the patient was dismissed well three weeks subsequent to the second amputation.

The *Transactions* close with an obituary notice of Dr. Nathan Knepfler, an Hungarian physician, who settled at Indianapolis in the year 1853, and by his moral character and kindly deportment as a man, his well cultivated mind, his varied knowledge, and his high professional attainments and capabilities, won the esteem and confidence of an extensive circle of friends and patients. He died, January 10th, 1859, at Shelbyville, Ky., where he had gone on a visit to his son. He had attained his fifty-sixth year at the period of his death.

4. With the exception of simply the minutes of the proceedings of the session of 1859, and the address of the retiring President, the publication issued by the *Medical Society of North Carolina* presents none of the scientific transactions of its last session. The reports from the several county societies; the papers on different subjects read by members, together with the address of Dr. J. J. Summerell, the Orator of the Society, were all referred for publication in the *Medical Journal of North Carolina*, a periodical published under the auspices of the Society.

The subject of Dr. Summerell's address was, some of the qualifications which should characterize the members of the medical profession: the position being eloquently enforced, that, while a thorough knowledge of the science and the art of healing is essential to constitute the skilful practitioner, not less essential are a polished demeanor, a high moral character, and a uniformly religious life, to the formation of the true medical gentleman.

The State Medical organization of North Carolina dates only from the 27th of January, 1849. Its subsequent progress has been one of constant and increasing prosperity. Though feeble in the commencement of its career, it is now recognized as the legally organized organ of the profession throughout the State, and includes among its members the greater portion of the respectable medical practitioners of North Carolina. In the fulfilment of the objects of its organization, it has succeeded in inducing those who would otherwise have withheld their quota from the general fund of medical knowledge, to embody their clinical observations in proper form, and to present them, through the pages of the Journal of the Society, for the benefit of their contemporaries and successors, and in this manner, it has contributed to the advancement of medical knowledge; by exercising a constant, judicious oversight over the conduct of its members, it has added its influence most effectual to the elevation of the character and standing of the profession, while by enlarging, through the reports and medical communications made to it, and through it to the public, our knowledge of the character, causes, pathology, prevention, and treatment of the various endemic, epidemic, and sporadic diseases, which the physicians, located in different sections of the country, are called upon to contend with, the Society has not failed in the accomplishment, to a good extent, of one of the great objects of its mission, the protection, namely, of the health, the promotion of the comfort, and the prolongation of the lives of the community. And, finally, by bringing together, at stated intervals, physicians from all the counties in the State, and under the most favourable opportunities for their becoming acquainted with

each other, and for the interchange among them of professional and personal courtesies, the Society has established that harmony and unity of action between the members of the profession generally, throughout the State, upon which so essentially depends the security of their individual interests, and the triumph of the entire profession over those evils with which it has had so long to contend.

5. The *Transactions of the Connecticut Medical Society*, for 1859, open with an address from the President, Dr. Ashbell Woodward, of Franklin. The address is devoted to a history of medical organization in the State, but especially of the State Society, from its commencement in 1792 to the present time. From it we glean some very interesting reminiscences of the state of medical education, and of domestic medical literature in the early days of Connecticut, and the other New England States.

From their settlement in 1620, to the organization of the Connecticut State Medical Society, a period of more than 170 years, no systematic effort had been made throughout the New England States, to elevate the grade of medical education, or to regulate the practice of the profession, with the exception of the organization of a State Medical Society in Massachusetts, in 1781, of another in New Hampshire, in 1791, and of some two or three voluntary county medical associations in Connecticut.

During the whole of the seventeenth century, it was only in the larger and first settled of the towns, that a few able and educated physicians were to be met with. Among these stood pre-eminent the Hon. John Winthrop, who, in 1662, was made the first Governor of the Colony under the charter which he obtained from Charles II. He was an eminent physician, a ripe scholar, and a liberal patron of science in general. He was not only a member, but one of the founders of the Royal Philosophical Society. His field of practice was first, New London, and afterwards, successively, New Haven, and Hartford. He died in 1676. In the year 1652, Dr. Thomas Lord, of Hartford, obtained from the General Court the first license granted by it for the practice of physic and surgery. Two years later, Dr. Daniel Porter, of Farmington, likewise procured a similar license. Dr. Porter attained to considerable celebrity as a practitioner, and, for more than thirty years, was one of the leading physicians in the colony. Dr. Thomas Hooker, of Farmington, and Dr. Samuel Mather, of Windsor, were licensed towards the close of the seventeenth century. It is believed that both enjoyed the private instruction of Dr. Porter. Neither of them was less distinguished in the medical profession than their eminent preceptor.

At a still later date, it appears that the leading physicians of New England, but especially of Connecticut, were members, also, of the clerical profession, who, during their collegiate course, acquired a knowledge of medicine from the writings of Hippocrates, Galen, and other early authors. Of this class of physicians were the Bulkleys, father and son, of Wethersfield and Colchester; Elliott, of Killingworth; Fisk, of Haddam, and Collins, of Litchfield. The first two were graduates of Harvard, the remaining three of Yale. Some of them, particularly Elliott and Fisk, were physicians of great eminence, and were not unfrequently called, in consultation, into the adjoining colonies.

The earliest medical publications appeared in Massachusetts, and were called forth by the prevalence of epidemic disease. The first, published in 1677, was a tract entitled, "A Brief Guide in the Smallpox and Measles," written by Thomas Thatcher, a clergyman and physician. Other publications of this early period, characterized by great and varied learning, accuracy of observation, and originality of thought, and which would do credit to a much later age, might be here enumerated. Thus, the "Practical History of a new Epidemic Eruptive Miliary Fever," which prevailed in New England, in 1735–36, by Dr. William Douglas, of Boston, published in 1736; a "Description of American Yellow Fever," by Dr. John Lining, published in 1753; and a work on the "Treatment of Wounds and Fractures," by Dr. John Jones, published in 1776, have had a high and enduring reputation.

"Dr. Benjamin Gale, of Killingworth, appears to have been the earliest medical writer in Connecticut. He studied medicine with the distinguished Dr. Jared Elliott, whose daughter he subsequently married. About the year

1750, he published a work entitled 'Historical Memoirs, relating to the practice of Inoculation for Smallpox in the British American Provinces, particularly in New England.' In this work the author advocated the utility of a course of mercury to prepare the system for the disease. This production has been referred to by the celebrated Dr. Huxham, who noticed with favour the practice recommended by the author. Dr. Gale also published some essays in the transactions of the original New Haven County Medical Society. These, with 'Cases and Observations,' published by the same Association in 1788, a work which has been referred to by foreign authors, and which gives a judicious view of the practice of the State of Connecticut subsequent to the war of independence, will, if we include a work on 'Pestilence,' by Noah Webster, LL. D., comprise the main part of our medical literature anterior to 1792."

The use of mercury in the treatment of inflammatory diseases and eruptive fevers, had its origin, we are told, with Dr. Wm. Douglas, of Boston, in 1736. The preparation used by him was calomel.

Up to the middle of the last century, and in some of the more sparsely settled towns, to near the beginning of the present century, the practice of midwifery was left almost exclusively in the hands of females. Dr. James Lloyd, of Massachusetts, and Dr. William Shippen, of Pennsylvania, were the first regular and successful male practitioners of midwifery in this country.

Among the most distinguished American physicians educated in the schools of Europe, who flourished at this early period, were Drs. James Lloyd and Zabdiel Boylston, of Massachusetts; St. Gad. Colden, and Dr. James Ogden, of New York; Drs. John Morgan, Wm. Shippen, Jr., and Benjamin Rush, of Pennsylvania; Drs. John Mitchell and Thomas, of Virginia; and Dr Lining, of South Carolina.

"The first public attempt to communicate medical instruction in America was made in 1754, by Dr. William Hunter, of Newport, R. I. He gave lectures on anatomy in that and the two succeeding years. He was educated under the elder Monro, at Edinburgh; was a contemporary of Cullen, with whom, as with his own illustrious kinsmen, Wm. and John Hunter, of London, he corresponded after his removal to this country. In the year 1762, Dr. William Shippen, who had then just returned from his studies in Europe, commenced a course of lectures on anatomy, to a class of twelve students, in the city of Philadelphia. The same course was continued for the two following years, when, in 1765, he was joined by Drs. John Morgan, Benjamin Rush, and others, in founding the medical department of the University of Pennsylvania."

In the winter of 1768–9, the first course of medical lectures was delivered by the medical faculty of King's College, New York. This faculty included, with others scarcely less distinguished, the names of Drs. John Jones, Peter Middleton, and Samuel Bard.

In the winter of 1780, Dr. John Warren, then a surgeon in a military hospital in Boston, commenced a course of anatomical lectures, which were annually continued until a medical faculty was organized in connection with Harvard University in 1782. But so slow were its beginnings, that for the next eighteen years but nine students were honoured with the degree of M. B., and not one with that of M. D.

"These schools, thus ably organized, were at first not well sustained. Six years after the organization of the New York school, only about twenty-five persons attended anatomical lectures, and some of these were from the West Indies. Not a single name from Connecticut is to be found upon the earlier catalogues of any of the institutions just referred to. It should be borne in mind, however, that the first medical honours were bestowed upon but few, and that even at a later date, they were much less sought after than at present."

"The medical students of Connecticut, prior to the organization of the State Medical Society, had no other than private medical instruction. There were, it is true, some competent and highly popular medical teachers scattered through the State, by whom large numbers of our young men were successfully educated. Among the most eminent were Dr. Jared Elliott, of Killingworth, who has justly been regarded as the father of regular practice in Connecticut; also, Dr. Jared Potter, of Wallingford, himself a student of Dr. Elliott, who for many years

kept a medical school, in which several of the most distinguished physicians in the State were educated, Dr. Lemuel Hopkins, of Hartford, being of the number. Another, scarcely less prominent, was Dr. John Barker, of Franklin, who was the first President of the New London County Medical Society, organized in 1784, to which office he was annually re-elected to the time of his death, in 1791."

"New Jersey is entitled to the credit of making the first attempt to improve the condition of the profession by means of social organization. As early as 1766, the profession in that State formed a voluntary association, which continued in successful operation till 1775, when its meetings were interrupted for a time by the military operations in that colony. This society did not obtain an act of incorporation till 1790. The second important movement in this direction was, as we have seen, in Massachusetts, when, in 1781, she received an act of incorporation for her State Medical Society. The next State society was in New Hampshire, which was chartered in 1791."

The address of Dr. Woodward is followed by a very elaborate dissertation on the *therapeutical application of the seton*, by Dr. Rufus Barker, of Deep River. We cannot follow the writer throughout the series of facts and arguments he presents, nor the practical lessons he deduces from them. The following are the general conclusions at which he has arrived in respect to the curative powers of setons, issues, rubefacients, etc. 1st. That, when in cases of inflammation, the indication is to produce revulsion or counter-irritation, those articles should be selected which produce no stimulating effect on the deep-seated organs by absorption, but act directly and solely on the parts to which they are applied, such as the seton, actual cautery, hot water, etc. 2d. That, in diseases of mucous tissues, tubercular depositions, and all inflammations of an asthenic character, counter-irritation is to be produced by those articles from which a greater or less amount of acrid principle will be absorbed. Dr. B. thinks it more than probable that issues, by their stimulant effect upon the terminal lymphatics of the skin excite the glandular and lymphatic systems to a healthy action, and thereby prevent the formation of scrofulous matter in the system. 3d. That, if disease is produced by faulty nutrition and excretion, whereby morbid matters are retained in the blood, and act as poisons, or are deposited on some imperfectly nourished organ, producing in it destructive inflammation, the leading indication is to remove this morbid matter from the system, and facts abundantly prove that a suppurative discharge or drain from the circulation does, very often, effectually and speedily fulfil this indication.

It is very certain that, in the course of his dissertation, Dr. B. has emitted many very excellent therapeutical precepts; these, however, are mingled with others that are either based upon hypotheses of very doubtful validity, or are inconsistent with generally received and well-established views in respect to the nature of certain forms of diseased action, and the means best adapted for their removal.

A well digested report on the sanitary condition of Hartford County, during the year 1858, is presented by Dr. A. W. Barrows. Though replete with interest to every physician within the county to which it refers, the report contains few facts and principles of interest to the profession generally.

In the report from the same county for 1857, attention was directed to the fact that the percentage which the deaths from consumption bear to the entire mortality, in the towns bordering upon the Connecticut River, with the exception of Hartford, was nearly or quite double to what it was in the more hilly portions of the county; during 1858, however, a very different result is exhibited, the percentage of deaths from consumption having been greater for the towns remote from the river, than for those bordering upon it.

In Southington, we are told that "by far the most prevalent disease is consumption;" it having been the cause of *one-third* of the deaths which occurred in the town during 1858. The following are assigned as the causes of the extensive prevalence of the disease in this portion of Connecticut; namely: 1st. Hereditary predisposition. 2d. Climate. 3d. Intemperate living; with the remark, that the last is doubtless the most common of the exciting causes of consumption, as it is of disease generally.

A report from the New Haven County Medical Society announces and deplores the entire failure of an effort that had been set on foot to induce the physicians of the county to preserve, according to a prescribed uniform system, and in blanks furnished to them, a correct register of all the diseases and accidents attended by them, together with their respective results, with a view to the formation of a sufficiently extended and authentic series of medical statistics for future reference. Could such a registration as that contemplated by the New Haven Society be effected, it would be possessed of a value which even they would recognize who now, by withholding their individual efforts in carrying it out, prevent its accomplishment.

A poem is next inserted, which was read by Dr. H. W. Painter, before the New Haven County Society, as a substitute for a scientific paper he was expected to present. The poem has more than one good point, but still we must say that we hold it to be sadly out of place where it now appears—in the midst of the published professional transactions of a State Medical Society.

The history of a case of complete laceration of the perineum, occurring during labour, in consequence of mismanagement on the part of the attending physician, and successfully operated on at the end of nearly thirteen weeks from its occurrence, by Dr. Orson Wood, by properly preparing the cicatrized edges of the rupture to enable them to unite when approximated, destroying the disturbing influence caused from the action of the sphincter ani muscles by its complete division, retaining in contact the ruptured parts by means of the quilled suture, and applying continuously the cold water dressings to the entire and perfect consolidation of the parts, which occurred at the end of two months from the period the operation was performed. The case is throughout one replete with instruction. An abstract of it would, however, scarcely convey a fair idea of its peculiar features, and of the exact nature and propriety of the operation instituted by Dr. Wood, with the after-treatment, etc., while it would occupy too much space to insert the entire history of the case and operation given in the Transactions.

Biographical notices next follow of four of the deceased members of the society. After this we have two appendices, the one being a preamble and resolutions, adopted by the Medical Society of the city of Hartford, affirmative of the claim of Dr. Horace Wells, as the discoverer and demonstrator of the use of anæsthesia in surgery; the other, a memorial to the Legislature of Connecticut in reference to the proper treatment of insane inmates of the prisons.

D. F. C.

ART. XXIII.—*Reports of American Institutions for the Insane.*

1. *Of the Vermont Asylum, for the fiscal year* 1857–8.
2. *Of the New York State Asylum, for* 1858.
3. *Of the Bloomingdale Asylum, for* 1858.
4. *Of the King's County (N. Y.) Asylum, for the fiscal year* 1858–9.
5. *Of the New Jersey State Asylum, for* 1858.
6. *Of the Mount Hope Institution, for* 1858.
7. *Of the Western Asylum of Kentucky, for* 1856 *and* 1857.
8. *Of the Indiana Hospital, for* 1858.
9. *Of the Illinois State Hospital, for* 1857 *and* 1858.
10. *Of the State Hospital of Missouri, for* 1857 *and* 1858.

1. THE new building for the *Vermont Asylum for the Insane*, which was in course of construction at the time of the annual report for 1856–7, has been completed, and is said to be greatly superior to the original edifice.

The barn belonging to the establishment was burned on the fifth of December, 1857, and the fine stock of the farm, consisting of twenty-nine cattle, were destroyed. It is supposed to have been the work of an incendiary.

The farm has been increased by the purchase of a considerable tract of adjacent land.

	Men.	Women.	Total.
Patients in the asylum, August 1, 1857 . .	192	221	413
Admitted in course of the fiscal year . .	89	68	157
Whole number	281	289	570
Discharged, including deaths	74	81	155
Remaining, August 1, 1858	207	208	415
Of those discharged, there were cured . .			80
Died			39
Whole number of patients, since opening . .			2869
Discharged, recovered			1366

Dr. Rockwell makes some brief remarks upon the utility of manual labour as a restorative agent, in which he says: "Whether the patients will cheerfully employ themselves depends, in a great measure, upon those who have the care of them. So much depends on the qualifications of the attendants, that too much caution cannot be exercised in their selection."

2. Those portions of the edifice of the *New York State Lunatic Asylum* which were destroyed by fire, in July, 1857, have been thoroughly rebuilt, with every practicable safe-guard against the occurrence of a similar disastrous accident. "The general sympathy and prompt aid extended toward the institution, and its early restoration, have enabled us," remarks Dr. Gray, "to pursue its objects with the least possible embarrassment, and with satisfactory results."

	Men.	Women.	Total.
Patients in the asylum, Nov. 30, 1857 . .	238	213	451
Admitted in course of the fiscal year . .	172	161	333
Whole number	410	374	784
Discharged, including deaths	150	132	282
Remaining, Nov. 30, 1858	260	242	502

The subjoined analysis of the cases admitted is such as ought, for the full elucidation of the nature of the disorder, and of the results of hospital treatment, to accompany the record of admissions in every annual report emanating from institutions for the insane. Who, without some exposition of the kind, can ever arrive at an opinion bordering upon accuracy, of the actual curability of mental disorders?

"Of the 333 admitted, 44 were readmissions. Of these, 32 for the second, 8 for the third, and 4 for the fourth time. Twenty-six of the readmissions have, at various periods during the past 16 years, been discharged recovered. The 26 were insane and under treatment an aggregate time of 37 years, and since the first attack, well and discharging the duties of life, an aggregate period of 159 years; showing that even in cases of reattack there is much to reward care and treatment, and to inspire hope both in the afflicted and in their friends.

"Of the 32 received for the second time, 20 were acute and 12 chronic cases. Of the 8 received for the third time, 4 were acute and 4 chronic; and of the 4 received for the fourth time, 2 were acute and 2 chronic cases. The 4 acute cases which were admitted and discharged three times as recovered, were insane at long intervals, and each attack was consequent on the breaking down of health from hard work, loss of sleep, and the debility following acute diseases. The aggregate of the attacks and treatment in these 4 cases was 6½ years, and the aggregate of health, from the first attack, 37½ years."

Causes of death.—Phthisis pulmonalis, 6; exhaustion from mental disorder, 13; general paralysis, 4; old age, 2; meningitis and softening of brain, syncope, suicide, abscess, paralysis, and typhoid pneumonia, 1 each.

In one of the cases terminating fatally from phthisis, "the exacerbations of pulmonary disease were characterized by a corresponding improvement in the mental condition."

In each of the four cases of general paralysis "the disease existed from three and a half to four years; in three, the hesitancy and thickness of speech peculiar to the disease being the first recognized symptom."

"Three of these cases were residents of the asylum from the commencement

of the attack of paralysis. The mental impairment became most prominent, and the characteristic delusions more marked about eighteen months before death. General exaltation of the imaginative faculties, and physical impairment and more manifest paralysis were ushered in by epileptiform convulsions. In two of the above cases the assigned exciting cause was intemperance and debauchery. The third had always been a sober and exemplary man. Of the earlier history of the fourth case, comparatively little is known, except that his habits were temperate, and his disease was excited by domestic troubles. He was seventy years of age, and lived but a week after admission."

The case of suicide was that of a woman who had been discharged recovered from a first attack of insanity, in which she had exhibited no suicidal propensity. "Several months after her readmission she was visited by her husband, who, in conversation with her, unguardedly expressed much disappointment at not finding her improved. On the same night she committed suicide by suspension."

In the following extract Dr. Gray treats of the causes of mental disorder which were greatly augmented and more widely prevalent than usual during the period included by his report:—

"Two remarkable and important events have marked the social history of our State and country during the past year; the financial panic, and the great religious awakening. * * * The panic has not largely increased the number whose insanity is assigned to "business perplexities."[1] For the six years previous, the average from this cause was nine and two-thirds; that is, for 1852, 7; 1853, 12; 1854, 15; 1855, 11; 1856, 10; 1857, 3. These figures show much uniformity for the past seven years, with the exception of 1857, a year of unusual seeming prosperity. The financial crisis, during its height, undoubtedly increased the number of suicides from causes unconnected with insanity, but this soon passed off, and its morbid effects upon the public mind were exhibited in other and secondary causes, such as intemperance, excessive labour, and in some cases, perhaps, in religious excitement; as it is well known, there has been an intimate relation of cause and effect in the healthful operations of the two events.

"The present revival has been peculiarly free from certain characteristics of former ones, and from the highly-wrought and extravagant appeals to the passions, the hopes and fears which have marked various epidemics of religious fanaticism. Although the number of cases in which religious excitement is the assigned cause, falls much below that reached by Millerism, spiritualism, etc., during the prevalence of those epidemics, it is further to be said that in at least one-half of the former, loss of sleep and mental exhaustion were the causes of insanity in persons already church-members, engaged in behalf of others, but themselves under no special excitement or concern. In nearly all the other instances included under this cause, as, indeed, under most of the so-called causes in the list, it is to be remembered that the agent is a purely exciting, rather than an efficient one, operating upon already prepared and smouldering elements of disease. It is also remarkable, and illustrates the absence of dogmatic and denunciatory tendencies in this revival, that no case of melancholia is among the number assigned to this cause."

Notwithstanding the conflagration before mentioned, and the inconveniences and embarrassments necessarily attendant upon the reconstruction of that portion of the edifice which was destroyed, the extensive measures for moral treatment long since adopted at this institution—the manual employment, the fairs, the musical, theatrical, and other entertainments—have been continued as heretofore.

Patients admitted from Jan. 16, 1843, to Dec. 1, 1858 . .	5,516
Discharged recovered	2,226
Died	636
Not insane	39

According to the report before us the county superintendents of the poor, in the State of New York, have for several years "held annual conventions to consider the condition and wants of the poor and insane, and by unanimous

[1] The number in 1858 was 12.

voice have declared the policy of keeping lunatics in the county houses, and associated with the ordinary paupers, at variance with justice, humanity, and public economy. They have also memoralized the legislature on the subject, and a few months ago, at an annual State convention, reaffirmed their experience and opinions against the receptacles, and appointed a committee to attend and represent to the legislature the condition and wants of the insane poor, and urge the speedy erection of suitable asylums for all the insane requiring special care or treatment.

"The good results of this movement on the part of the superintendents of the poor, and the investigations of the senate committee into the state of county houses and receptacles for the insane, are continually coming to our notice. These buildings have, in several instances, been made more comfortable and commodious, and are constantly gaining in a spirit of liberality and humanity of management."

Another no less creditable and worthy movement of these superintendents of the poor, is that of urging the passage of a law "directing the removal of all dependent children from the poor-houses to orphan asylums," where they may be properly educated. As an evidence of the necessity for some statute provision of this kind, Dr. Gray says: "The number of persons coming under our care and destined to continue a public charge during life, whose history from childhood reveals that their insanity is owing to their being neglected while orphans, vagrants, or intemperate, or worse than neglected by being confined in idleness, and amidst immoral associations, is sufficient to afford a subject for extended discussion."

3. The record of the *Bloomingdale Asylum*, for the year 1858, furnishes the following results:—

	Men.	Women.	Total.
Patients at the beginning of the year . . .	67	79	146
Admitted in course of the year	55	57	112
Whole number	122	136	258
Discharged, including deaths	59	54	113
Remaining at the end of the year . . .	63	82	145
Of those discharged, there were cured . .	17	17	34
Died	4	7	11

Causes of death.—Pulmonary consumption, puerperal mania, exhaustion of acute mania, suicide, gradual exhaustion in an aged patient, prostration after typhoid fever, apoplexy, epilepsy, general paralysis, dysentery, and disease of brain attended by convulsive paroxysms, one each.

Dr. Brown remarks that, to the pecuniary disasters of the past year, "is to be attributed the untimely removal of a large proportion of the patients discharged without recovery, or even without decided benefit. * * * Of the thirty-four patients who left improved, but seventeen were regarded as favourable for treatment at the time of admission, and of these eight recovered at home after leaving the asylum."

Gas, for lighting the apartments, has been introduced. "By donations from members of the asylum committee the walls are being decorated with a class of pictures and engravings rarely seen in such apartments."

4. The rapid increase of the population of Brooklyn, New York, has been accompanied by a corresponding augmentation of patients at the *King's County Lunatic Asylum*. If we mistake not, it is but about fourteen years since the first building, separate from the general almshouse, was erected for the insane. Four or five years ago, that was abandoned for a new and much larger edifice. It was not long before it became necessary, from the increase of patients, to place a portion of them in the old building; and now both are occupied to their utmost capacity. The enlargement of the new asylum, by additions which will complete the original design, is recommended in the report under notice.

	Men.	Women.	Total.
Patients in the asylum, July 31, 1858 . .	108	160	268
Admitted in course of the year	93	94	187
Whole number	201	254	455
Discharged, including deaths	79	86	165
Remaining, July 31, 1859	122	168	290
Of those discharged, there were cured . .	39	47	86
Died	12	8	20

Dr. Chapin discusses the effects, as generative causes of mental alienation, of the modern system of education, and of the excessive strain upon the intellectual powers of adults, to meet the exigencies of the general style of living at the present day. The substance of his remarks has already been given, in our quotations from the reports of other similar establishments. He treats, however, another subject which, so far as we recollect, has not engaged the pen of any other superintendent, in his annual reports. We allude to the effect of the adulterated liquors now so extensively used. It is to be hoped that we shall have the results of observation at other institutions of the kind, in regard to this matter.

"Intemperance has long been recognized as one of the most prolific causes of insanity, but the impure wines and liquors in common use at the present day, have increased the prevalence and somewhat changed the character of this form of mental derangement. Probably but a small portion of the community have any conception of the extent of the comparatively modern iniquity of adulterating alcoholic stimulants, and the ascendency such adulterations have gained over pure liquors, in destroying the health, and the moral character, and the mind itself of persons addicted to the use of intoxicating beverages. It is not alone those known as fast men, inebriates, that are exposed to these dangers, but the light drinkers, the moderate men—who indulge only occasionally in stimulants—are also liable to be deprived of their senses at any moment, or to become so unnaturally excited, or depressed, or confused, as to be impelled to drink to excess, and madness is often the speedy or more remote result. Patients are sometimes brought to the asylum, presenting the appearance of having been intoxicated for weeks; but, upon subsequent inquiry, it is found that they had been pursuing their avocations up to a few days previous with sober regularity. Some of them, perhaps, were addicted to occasional frolics or sprees, and the result is less surprising to their friends than the suddenness of it. Others were never known to be under the influence of, much less incapacitated by the use of liquor. Not unfrequently the unfortunate persons have wandered from their homes, and nothing can be ascertained of their history or former condition, as they are seldom able to give any reliable account of themselves. The officer charged with their commitment, has found them in some station-house or jail, where they have tarried for a longer or shorter period, until the fact of their insanity could be legally established. While in these places, they are often represented as having conducted themselves in the strangest manner, cursing and praying alternately, tearing their clothing, mutilating their persons, quarrelling with those who would administer to their necessities; in short, raving maniacs indeed, such as old times' descriptions and some modern imaginations make every insane person to be. These more violent manifestations of the disorder are generally relieved in a short time after entering the asylum, and, in fortunate cases, the mind is restored in the course of a few weeks or months. In other cases the excited or paroxysmal stage continues with greater or less vehemence for many months, when reason may slowly return, though often leaving the mental faculties for a long period in a feeble condition. Frequently great mental prostration and depression succeed the excitement generally manifested on entering the institution. This condition is likely to be one of long continuance, and the mental anguish is often so great and unendurable as to impel to frequent attempts at self-destruction. But too often reason is irretrievably lost. This form of mental alienation also frequently terminates in general paralysis."

The institution is gradually enlarging its facilities for the moral treatment of its inmates.

5. The general statistics of the *New Jersey State Lunatic Asylum*, for the year 1858, are as follows:—

	Men.	Women.	Total.
Patients at the beginning of the year	135	144	279
Admitted in course of the year	72	75	147
Whole number	207	219	426
Discharged, including deaths	59	74	133
Remaining at the end of the year	148	145	293
Of those discharged, there were cured	30	32	62
Died	9	7	16
Not insane	1		1

Causes of death.—Exhaustion from acute mania, 4; softening of the brain, 1; erysipelas, 1; general or chronic exhaustion of the system, 3; consumption, 3; epilepsy, 4.

"Of the first four deaths it should be remarked that they occurred, as deaths from acute exhaustive mania usually do, soon after admission, the disease having nearly run its course and exhausted the powers of life before the arrival of the patient at the institution.

"In the preliminary treatment of such cases, it should always be kept in mind, that the high mental excitement, attended by extreme watchfulness and desire for muscular exertion, depends on a highly *irritable*, rather than on an inflammatory condition of the system. Hence direct depletion, drastic purgatives, blisters, etc., tend rather to increase than to allay the effect of the primary injury."

Dr. Buttolph relates the following interesting case:—

"Mrs. ———, aged about forty-five, had an attack of derangement in early life, which continued seven years, but from which she recovered quite perfectly, and remained well a number of years, during which she married. In February, 1848, she was admitted to this asylum, having recently had a second attack. She was then in a highly excitable state of mind, and so remained much of the time for nine years, having only short intervals of comparative quiet once in a few weeks. She constantly held the delusion that she was a queen, and that those about her were her subjects. Her speech and motions were rapid, and her habits generally filthy and destructive, especially in regard to clothing. She slept very little at night, and usually passed her time in rearranging the straw in her bed, talking, or rather chattering, pounding, etc. Would sleep or doze for brief periods during the day, often in the sitting posture, which seemed sufficient to prepare for the labour of the next night.

"During the early part of the year, and occasionally before, she had longer intervals of quiet and comparative rationality. In the month of March she had a severe illness, which lasted several weeks, and reduced her to the verge of the grave. This illness was characterized by a feverish state, loss of appetite, flesh, and strength. There finally appeared in various parts of her body and limbs, and about her head, a series of sloughing boils, which seemed the climax of her physical disease. From this state she slowly improved, having only now and then slight indications of her former mental disorder. She remained with us several months after she appeared quite well, to make sure of a permanent restoration. She then left in full flesh, health, and hope, having nearly completed her tenth year in the asylum."

There are more patients at this asylum than at any former period, and more than can be easily accommodated.

	Men.	Women.	Total.
Patients from May 15, 1848, to Jan. 1, 1859	660	717	1377
Discharged recovered	248	281	529
Not insane	1		1
Died	91	89	180

The method of forced ventilation, by a revolving fan, was introduced into the asylum the past year.

6. From the Sixteenth Annual Report of the *Mount Hope Institution*, we take the subjoined numerical exposition of the results in the medical department of that establishment during the year 1858:—

	Men.	Women.	Total.
Patients at the beginning of the year . . .	58	79	137
Admitted in course of the year	58	59	117
Whole number	116	138	254
Discharged, including deaths	56	54	110
Remaining at the end of the year . . .	60	84	144
Of those discharged, there were cured . .	24	19	43
Died	8	4	12

"Two of the male patients, labouring under febrile delirium, died within three days after admission, and three died of paralysis. A number of the deaths were of patients at an advanced age, one being seventy-two years of age, and another seventy-six."

Besides the foregoing, 21 cases of mania-à-potu were admitted. One of these patients died, and one remained in the hospital at the close of the year. All the others were discharged recovered.

Of the patients received, thirteen had the suicidal propensity, but no one succeeded in effecting his fatal purpose.

Dr. Stokes attributes the failure, in many instances, to resort to hospitals at a sufficiently early period, to a prevalent "ignorance of the incipient symptoms of mental disorder."

We quote from this elaborate report such extracts as appear to us of the greatest value to those for whose information or entertainment we devote these pages.

In the table of causes of mental disorder "ten cases are classed under the head of *oinomania*, or *insane drinking*. It would really appear as if persons affected with this form of *moral insanity*, are at times actuated by a morbid craving for stimulants which they are incapable of resisting. No sooner do they begin to use their old enemy in moderation than their morbid appetite is regenerated, and immoderate drinking, uncontrolled and incontrollable indulgence, plunges them into a vortex which they seem to have no power to escape. They do not drink from the pleasure which the social board affords, nor for the pleasure which the wine gives them, but they will drink any kind of intoxicating liquor; and will drink as often as they can, and whenever they can, and as much as they can. Their craving for stimulants becomes incessant and incontrollable. No considerations of self-respect, no regard for public opinion, or common decency, or domestic ties, or religion, or the certainty of impending ruin, degradation, or even the fear of death, can prevent their drinking till they can drink no longer. In fine, such persons seem for the time to be deprived of the power of self-control, and should be regarded as no longer responsible agents, and therefore proper objects for being restrained, and protected against themselves." * * *

"There is a prejudice abroad that lunatics should not associate with lunatics; and that such mutual intercourse has a tendency to aggravate the disease. This is a fallacy. Nay, strange as it may appear, the delusions under which the insane labour are sometimes corrected and dissipated by the sallies of wit and repartee of their fellow patients. Thus we have observed impressions arise out of incidental conversations, which have at once awakened rational reflection, and suddenly a light has broken in upon the mind, which, like a heavenly ray, has appeared to guide reason *back to her deserted throne*." * * *

"In every asylum there will be found a class of discontented spirits who take an insane pleasure in presenting and describing, with the most exaggerated statement, their wrongs and grievances. In such cases it is extremely difficult to combat or allay, by appeals to patience and resignation, or by the distractions of occupation and recreation, their unfortunate propensity. With some this morbid feeling comes on in paroxysms, and continues for a short time, during which they take every opportunity of diffusing the querulous, captious, and dissatisfied tone of their own mind, to all around. In particular forms of insanity,

such as the *manie raisonnante*, for example, of Esquirol, this morbid state of irritability is very remarkable, and such patients only require an uncontrolled access to the different wards to produce an insubordination and dissatisfaction to all within their reach. To indulge such patients in this morbid love for mischief-making is only to promote and aggravate their moral malady. They must be checked and controlled by a firm yet kind, a decided yet parental discipline and government, before a more happy and natural state of feeling and inclination can be restored."

The proprietors of the Mount Hope Institution have decided to erect a new establishment upon a farm of 360 acres, well watered, susceptible of great improvement and embellishment, and situated six miles from Baltimore, on the Reisterstown Road. The edifice will be in the Palladic Italian style, consisting of central building and wings, the united front façade of which will be 580 feet in length. At the time of the publication of the report the materials for one of the wings were in course of preparation.

7. The principal numerical results of the operations of the *Western Lunatic Asylum of the State of Kentucky*, for the two years 1856 and 1857, are as follows:—

	Men.	Women.	Total.
Patients in the 31st of December, 1855 . .	44	37	81
Admitted in course of two years . . .	61	34	95
Whole number	105	71	176
Discharged, including deaths	47	27	74
Remaining December 31, 1857			102
Of those discharged, there were cured . .	22	16	38
Died	14	4	18

Causes of death.—Pulmonary consumption, 5; congestion of the brain, 3; chronic diarrhœa, 2; enlargement of heart, epilepsy, erysipelas, exhaustion from acute mania, scorbutic hemorrhage, cancer, and pleuro-pneumonia, 1 each.

Dr. Annan ascribes "the general good health of the people, during the past three years, in a great measure to the use of soft water, free from lime, for drinking and cooking." He also states that, "there has not been any suffering amongst the patients for want of a sufficient supply of heat. Stoves, burning coal, have kept the day-rooms as warm as was necessary for comfort; and an abundant supply of bedclothes has prevented complaint during the night. There has not been a case of frost-bitten limbs."

The patient who died with erysipelas was epileptic. "His intellect, however, did not appear to be impaired, but he was irascible, easily excited, and then was inclined to fight. His only delusion was, previous to admission, that every person he associated with was his enemy. On examination of his brain," continues Dr. Annan, "several ounces of serum were found in the sac of the arachnoid membrane, and yellow pus was seen on the surface of both of the posterior lobes of the cerebrum. Between the convolutions of these posterior lobes there were several ulcers of the gray matter, which had destroyed it down to the white medullary substance. The gray matter around these ulcers was softened. Between the convolutions there was some brown semi-fluid matter. The entire surface of the brain was of a pale lead colour. The anterior and middle lobes were sound. No lesion was detected, except the altered colour.

"In this case we have the chief seat of disease in that part of the brain which is supposed to be appropriated to the feelings and propensities, while the anterior portion, the seat of the intellectual powers, was in a comparatively healthful condition. The mental manifestations during life accorded with the appearances after death.

"In all of my examinations, here and elsewhere, either the anterior, or the adjacent parts of the middle lobes of the cerebrum, have been more or less diseased when the intellectual faculties have been disturbed. This, as far as I have observed, has been invariable."

8. The *Indiana Hospital for the Insane* has again got into successful operation, and its report for the fiscal year terminating with the 31st of October, 1858, presents the subjoined numbers.

	Men.	Women.	Total.
Patients at the beginning of the year			71
Admitted in course of the year	160	150	310
Whole number			381
Discharged, including deaths			104
Remaining at the close of the year			277
Of those discharged, there were cured			80
Died			8
Aggregate of patients, 1849 to 1858, inclusive	785	765	1550
Discharged, recovered	361	363	724
Died			116

Causes of death the past year.—Maniacal exhaustion, 2; chronic meningitis, 1; œsophagitis, 1; phthisis pulmonalis, 2; tabes mesenterica, 1; suicide, 1.

"The diseases common to this country during the summer and fall months have prevailed to a very limited extent—so inconsiderable that there have been no deaths from this source."

Dr. Athon's report treats, under separate heads, upon a variety of subjects, most of which, being connected with the construction, improvements, or management of the hospital, are of but local interest. He recommends the enlargement of the building, by the addition of a wing. In the section devoted to the "Intermarriage of Races," he takes the positions that the mulatto is intellectually superior to his African parent, but is not so long-lived as either the white man or the negro; and that the offspring of intermarriage between the negro and any of the copper-coloured races may be, and sometimes is superior to either parent.

Treating of causes, he says: "In those districts where summer and autumnal diseases prevail, we expect, and we certainly find more cases of insanity arising from physical causes, than we do in localities more favourably situated for health; while on the other hand, the same may be said of moral causes, that wherever the intellectual faculties are exercised the most, and wherever enterprise and improvement prevail the most, there we find the greater proportion of the cases of insanity attributable to moral causes."

Of "spiritualism" he writes as follows: "That there are manipulations and mental exercises of an extremely exciting, if not an interesting nature, connected with spiritual manifestations, none, who have witnessed the operations of this sect, will deny. But whether this vaunted mode for communicating with the dead be true or not, it seems to us that we have everything to lose and nothing to gain, by pursuing a subject which in all its phases, presents mysteries doubly mystified by the ignorant and designing. The worshippers at the shrine of this great fanaticism comprise persons of every grade of intellect, from the mere imitators, to those whose minds grasp the cyclopedia of knowledge, and mould and warp the opinions of all who may come within their reach. * * *

"The effect of spiritualism upon the mind and body is almost irredeemably ruinous. The followers of this delusion exhaust in their "sittings," the nervous system. * * *

"The loss of sleep and the exhaustion of the body cause the mind to wander, incoherency supervenes, the "sittings" are persisted in, and these troubles, together with the extraordinary effort of the mind to catch the least tap of some favourite spirit, prove too much for the continuity of thought, the intellect reels, and what was once a rational being, now becomes a raving maniac."

And thus upon the use of tobacco: "Scores of men may and do use this narcotic plant without producing any perceptible effects, either on the mental or physical system, yet there are constitutions upon which it acts as a powerful depressing agent, manifesting its deleterious effects in nervous headache, tremor of the hands, want of appetite, dimness of sight, heartburn, ringing in the ears, listlessness, and inaptitude for any kind of exertion either in thought or deed. Individuals affected in this way become, sooner or later, careless, indolent, inco-

herent, and eventually insane, it generally appearing in the form of dementia, accompanied with paralytic symptoms."

In the subjoined extract, he touches upon a subject more fully discussed by Dr. Chapin. "We attribute the frequent occurrence of insanity from intemperate drinking to the strychnine, fishberries, and other poisonous drugs used in the adulteration of the various alcoholic preparations."

9. The operations of the *Illinois State Hospital for the Insane*, for the two years ending with the 31st of November, 1858, are thus represented:—

Patients in the hospital Dec. 1, 1856	214
Admitted in the two years	312
Whole number	526
Discharged, including deaths	297
Remaining, Dec. 1, 1858	229
Of those discharged, there were cured	164
Died	42

Causes of death.—Exhaustion from long-continued mania, 18; exhaustion from acute mania, 10; consumption, 6; epilepsy, 3; dysentery, typhoid pneumonia, intestinitis, dropsy, and effects of burns, 1 each.

"In those cases where a resort to the institution is concluded on after many months of ineffectual trial of other means we hear the universal regret expressed that the measure was so long delayed, showing that it is simply a matter of procrastination or an aversion to a step in itself somewhat unpleasant."

Dr. McFarland's report contains but little of special interest to the strictly professional reader. After a detailed description of their recently introduced apparatus for forced ventilation, he thus pleasantly gives the reasons for the necessity of so great a pecuniary sacrifice.

"If air would, like a civil but wearisome guest, rise up and go out when its presence becomes no longer tolerable, we should not have to go far for a remedy. But it is a fact in chemical philosophy that the more depraved the air becomes the less movable it is. A few shifty, fitful, and deleterious air currents let in at the chance openings of a door or a window, or the relief once vainly sought by certain openings left in the walls, fondly termed 'air-flues,' but along which air had no possible inducement to go, are all a mockery if relied on against the slow asphyxiation to which patients in a crowded lunatic hospital are inevitably exposed. The deplorable consequences of this state of existence are but too apparent to the observant physician. * * * The cheek loses its fresh tints, the eye its expression, and the whole countenance bears witness to the physical lassitude that has taken the place of the natural forces of the individual. But it is chiefly in certain latent consequences which do not reveal themselves to the eye, that the slow poison of a vitiated atmosphere shows its operation. Nature becomes imperceptibly disarmed of that conservative force by which the inroad of disease is averted. Fevers, naturally of a mild form, easily pass into the typhoid state; wounds of a trivial nature become erysipelatous; abscesses form without apparent cause, which are slow in maturing and healing; and, in short, that poverty of the constitutional forces which physicians term the 'cachectic state,' sooner or later prostrates the healthiest system."

One of the new wings of the building is completed.

To show how they do things "out west," we quote some items relating to the farming operations at this hospital.

Average stock of the farm, 8 horses, about 30 cows and neat cattle, and something more than 100 swine.

Products in 1858, in bushels, corn, 3,000; potatoes, 1,138; sweet potatoes, 60; tomatoes, 100; apples, 100; onions, 50; beets, 125; parsnips, 50; carrots, 30; cucumbers, 35; peas, 13; beans, 10. Hay, 85 tons; cabbage, 4,125 heads; squashes, 525; melons, 1,077; lettuce, 2,000 heads; radishes, 3,200.

10. In the fourth biennial report of the *State Lunatic Asylum of Missouri*, which embraces the two years ending on the 29th of November, 1858, we find the numbers usually transferred to our columns to be as follows:—

	Men.	Women.	Total.
Patients at the beginning of the two years . .	71	64	135
Admitted in course of the two years . . .	73	49	122
Whole number	144	113	257
Discharged, including deaths	47	39	86
Remaining, Nov. 29, 1858	97	74	171
Of those discharged, there were cured . .	25	20	45
Died	12	10	22

Causes of death.—Ulceration of the bowels, 8; chronic mania, 3; general paralysis, 3; consumption, 2; typhoid fever, 2; general dropsy, menorrhagia, suffocation from choking, and epilepsy, 1 each.

The deaths from ulceration of the bowels were of patients long insane, and, says the report, "might, perhaps, with as much propriety, have been referred to chronic mania. This form of intestinal disorder is a prominent indication of a general failing of the vital powers, and one of the most common terminations of protracted mental disease. If not, therefore, properly regarded as one of its symptoms, it is, doubtless, to a great extent, modified and aggravated by the want of healthy innervation."

We shall pass over some things which we otherwise might have noticed in this report, in order to present, unabridged, to our readers the following most excellent remarks by Dr. Smith, upon the subject of moral treatment:—

"I am persuaded we need, in most of our institutions, more of what Dr. Conolly has termed proper 'individualized' treatment; in other words, the influence of the sane mind acting beneficially upon the insane—when officers and attendants, in their daily intercourse with patients, impress upon them the influences of their own characters with a tact and skill that result in their improvement. Every insane person has his peculiarities, which require an adaptation of moral treatment, and hence the necessity of those whose duties bring them in immediate contact with the bereft of reason, first familiarizing themselves with the distinguishing characteristics of each. They should have largely developed the organ phrenologists term 'human nature,' which gives its possessor the power, almost at a glance, to discern the prominent traits of others, and the ability to adapt himself to them.

"How far we should resist the delusive opinions of the insane, has given rise to some difference of opinion. With few exceptions, all agree that it is never proper to elicit the expression of a patient's hallucinations, but, on the contrary, a prominent indication to remove, as far as practicable, every cause of perverted feelings, and excite those of a pleasant and antagonistic nature. A certain writer has truly said, 'A morbid thought not expressed, like a morbid feeling not expressed, will often fade away by the kindly influence of time.' It is also equally true, that any habitual state of feeling is increased and strengthened by permitting its free expression. How important, therefore, the indication in moral treatment, instead of eliciting, to repress the expression of a patient's delusions by gaining his confidence, fixing his attention, and presenting new and interesting subjects for reflection, that will stimulate to healthy thought, and divert from self-contemplation, until new trains of ideas become the habit of the mind, and the subjects of delusion, by imperceptible degrees, cease to exercise their dominion.

"There are some patients, however, who persist in obtruding their hallucinations upon notice so directly and pointedly, that dissent or acquiescence is unavoidable. In such cases, if regarded among the curable, I doubt not, as Dr. Bucknill, the distinguished English writer, has remarked, the only proper course is a positive denial, without assigning reasons. Whether the denial should be expressed in words or demeanor, would depend upon the disposition and excitability of the patient. No undertaking would be more futile than the effort to drive the insane from their delusions by arguments. They cling to them with a tenacity and steadfastness that time, death, or recovery alone will remove. A convalescent patient once said, 'No one who has not been insane can imagine how terribly real the delusions of lunatics are.'

"There are comparatively few cases where the necessity arises for direct

antagonism to the delusions of the insane, and especially when the officers and attendants possess that degree of skill, tact, and ingenuity, so essential to the triumph of moral influences."

The doctor thus discourses upon the subject of corporeal restraints: "I believe the period is not far in the future, when, with properly constructed buildings, the means for useful and varied employment, amusement, and mental occupation, will be so far increased, that, in connection with skilful, individualized treatment, it will be, comparatively, easy to abandon all mechanical restraint, and at the same time avoid protracted seclusion, equally, if not more objectionable. All well-conducted institutions are now evidently tending to this result, the consummation of which would constitute an additional triumph of science and philanthropy, worthy of the present age."

The asylum has been so crowded with patients that additional accommodations have been found necessary, and a new wing is in process of construction.

P. E.

ART. XXIV.—*An Introduction to Practical Pharmacy.* By EDWARD PARRISH, Principal of the School of Practical Pharmacy, &c. Second edition, with two hundred and forty-six illustrations. Philadelphia: Blanchard & Lea, 1859. 8vo. pp. 720.

WE welcome with pleasure this addition to our medical literature, and cordially commend its careful perusal to the junior members of our profession, whether practising in the cities or in the rural districts. To the latter, indeed, it is more particularly adapted, as it offers them the guidance of an able pharmaceutist who describes, in plain and simple language, the various processes and manipulations required in the preparation of medicines for the use of the sick. Most practitioners have felt at different periods of their professional career, and especially at its commencement, their unskilfulness and want of dexterity, whenever circumstances have compelled them to compound their own medicines. The work before us is specially designed by its author for the relief of such difficulties.

Part I. is devoted to the consideration of preliminary matters, such as the description of the various kinds of bottles, of weights and measures, and of specific gravities.

In Part II. we are introduced into the mysteries of pharmacy; so far, at least, as they are concerned in the manufacture of preparations. This portion of the work has strong claims upon the attention of the pharmaceutic student. In it the officinal preparations are arranged in groups; and under each head are embraced such unofficinal articles as have stood the test of time, or have been recently introduced into professional notice; the whole being preceded by practical instructions, and each particular group having the peculiarities of its manufacture carefully explained. As examples of the author's mode of treating his subject, we select the chapters on "Displacements" and on "Tinctures."

In these the different presses and vessels which are employed in exhausting the drug are first described; then follows an explanation of the process, with remarks on the methods of conducting it; and, lastly, the difference in results, determined by the nature of the materials operated on, is carefully explained.

The tinctures are divided into three classes: in the first, the menstruum used is diluted alcohol; in the second, it is alcohol; and in the third, it is the aromatic spirit of ammonia. A further subdivision into groups is made, but in this, we think, the author has not shown his usual skill; for no leading rule, whether pharmaceutic or therapeutic, seems to have been adopted.

Tinctures are grouped together which correspond in a single point, although differing in other and more important characters. Among the unofficinal tinctures at the end of the chapter, those of arnica, matico, strychnia, and Indian hemp, and the aromatic tincture of rhubarb, Dewees's tincture of guaiac, and Flemming's tincture of aconite, are introduced.

Part III. is devoted to the consideration of the relations of pharmacy to organic chemistry. Much labour has evidently been bestowed upon it, and the most important articles of the respective groups of organic products are arranged in a "syllabus" or tabular form, thereby facilitating the work of reference or comparison.

Part IV. points out the relations of pharmacy to inorganic chemistry, and furnishes to the compounder valuable formulas for the preparation of the phosphates, of the hypophosphites, and of numerous other salts which have been recently discovered or introduced.

Extemporaneous pharmacy forms the subject-matter of Part V. Three of its chapters are devoted to "prescriptions," "the writing of prescriptions," and "the art of combining medicines." These important but oft-neglected parts of a medical education are briefly and clearly explained, the author restricting himself to their practical exposition, and thus avoiding the tedious prolixity of the French writers who treat of the same subjects.

The appended prescriptions are designed "to illustrate the several advantages to be attained by medicinal combinations, and the means by which they may be most readily and safely fulfilled."

From the foregoing summary the reader will derive no incorrect idea of the general character of the Practical Pharmacy. Viewed as a whole, we find in it much to commend, and but little to condemn. Being, chiefly, a compilation from standard works of the highest authority on pharmacy, chemistry, and materia medica, it has just claims to a scientific rank; while the practical deductions and observations of the author render it particularly valuable to the practitioners of his art. The style is clear and unadorned, and the language is usually explicit. A close observer, however, is struck with the author's custom of using symbols and contractions instead of writing the words in full. The habit of confounding or compounding the Latin and English languages in prescriptions, insensibly acquired by all who are engaged either in prescribing or dispensing, shows itself frequently in these pages, as in such expressions as the following: "Take of *liquor ammonia*, sufficient." (p. 497.) "℞—Water *q. s.* to make f℥vij." (p. 497.) "Take of *pulv. pil. hydrarg.* gr. x." (p. 594.) "Take of *cortex pruni Virg. contus.* ℥xij." "Syllabus of *spiritus.*" (p. 267.) "*Spt. sal volat.* is a very useful antacid stimulant" (p. 462), &c. &c.

Surely these should not be thus expressed in "A Guide for the Physician," lest our profession continue in its present habit of careless prescription-writing. The third edition will present the formulas as correct in language as they now are in quantities and doses, or we are mistaken in the character of the author.

R. P. T.

ART. XXV.—*Report of the Select Committee appointed to Investigate the Health Department of the City of New York. Transmitted to the Legislature, February* 3, 1859. 8vo. pp. 211. Albany.

No municipal government can be said to fulfil faithfully the leading and most essential object of its organization—to secure the personal safety of the citizen, to promote his comfort, and to minister to his individual well-being generally—which makes no provision for his protection from those influences which tend to destroy his health, reduce his physical and mental energies, and curtail the length of his days, by suitable sanitary regulations enforced, by enlightened and efficient officers appointed for the especial purpose. No community is well governed where the laws ignore what science and experience indicate as the demands of health, and neglect to provide for the means by which human physical infirmities are diminished, and the prolongation of life, of enjoyment, and of usefulness promoted.

What avails it if the citizen be made secure in life and limb from the weapon of the assassin and the assault of the ruffian, if, at the same time, he is made the inevitable victim of an atmosphere that has been rendered poisonous from a

neglect on the part of the civic authorities to prevent accumulations of filth, and all sources of filth, the overcrowding of dwellings, the bad construction of public buildings, and the carrying on, in the midst of populous neighbourhoods, of unwholesome trades and occupations—from their neglect to secure free ventilation, the free admission of light, and a thorough system of drainage and sewerage at those points in which they are demanded. How much is the citizen benefited by the ample security thrown around his property by the law, if he is allowed to be continually robbed of his health and vitality, by unwholesome and sophisticated food, by unwholesome dwellings, and by a deficient supply of water for the purposes of cleanliness and cookery, and as a drink.

A wisely planned and judiciously and strictly enforced code of sanitary regulations, is as essential to the physical safety and well-being of the citizen as is the most excellent criminal code that can be devised, aided in its execution by the most efficient police regulations, and the most enlightened and incorruptible magistracy. In some respects it may be pronounced to be even more so, inasmuch as the aggressions upon the person and the property of the citizen, against which the criminal code is intended to provide, are more open, tangible, and appreciable, and consequently more readily avoided and guarded against by each individual than malaria and infection—those unseen, subtle agencies which prey upon his health and jeopard his very being, and which are often generated around him and in his very domicil, silently, slowly, and secretly—the first warning that they are present being when their morbific and deadly agency is exhibited in the production of pestilential disease, and in the mortality which follows in its train.

Few, if any, of our cities can boast of the possession of a sanitary code, the faithful execution of which is adapted to afford to the community the desired security of health, comfort, and life. Some of our larger cities suffer annually, to a degree truly appalling, from the want of such a code. It is true that upon the eve of some approaching epidemic, or when pestilence has made its appearance in the midst of a community, there is immediately set on foot measures which are supposed to be adapted to ward off the first, and to arrest the latter. But these spasmodic efforts for the institution and carrying out of sanitary measures, even when conceived in wisdom and prosecuted for the time being with sufficient vigour, seldom yield any results of a permanently beneficial character. Too generally, the moment the cause for alarm has passed by, the measures are relaxed, and very soon afterwards abandoned entirely. It is only by the unremitting and strict enforcement of sanitary measures that their full conservative agency can be obtained.

It is to be sincerely regretted that, in the cities of these United States, where everything was available for their proper arrangement, that in their original projection, or, at least, in their subsequent extension, more care had not been taken to secure for them the most favourable sanitary conditions, instead of cupidity, recklessness, and folly being allowed free license to transgress every ordinance of civic hygiene—rendering the work of reform, whenever undertaken, one of extreme difficulty, involving an incalculable expenditure of money, and demanding an abatement of nuisances, which, in many instances, from the length of time they have been practised, have come to be esteemed as legitimate rights.

The very decided movements that have been recently commenced in different portions of the United States, especially in her larger commercial cities, towards a methodical and thorough investigation into the condition as to health of the different classes of the community, the causes, if any exist, by which their health and vigour are impaired, and their chances of life diminished, are to be hailed as an indication that the people are becoming awake to the importance of an efficient system of public hygiene to their physical welfare, and that there is now a fair prospect presented of the early inauguration of such a system in every municipality throughout the Union.

Before us we have the report of a select committee appointed by the Legislature, to examine into the provisions and working of the health department and sanitary laws of the city of New York, and to recommend whatever legislation may be found necessary to increase their efficiency.

The committee, in the fulfilment of the duties intrusted to them, instituted at once a searching inquiry into the actual sanitary condition of the different sections of the metropolis, and finding, upon investigation, that the rate of mortality, in proportion to the population, was much beyond that of other large cities of the United States and in Europe, they were next led to examine into the general causes to which this excess of mortality is to be ascribed.

From the concurrent testimony of many of the most eminent physicians and other citizens of New York, given in detail before the committee, it was found that the causes to which the unfavourable condition of health which there prevails, and the excess of mortality beyond that of other populous communities, are mainly attributable, are the overcrowded condition of tenement houses, the want of practical knowledge of the proper mode of constructing such houses, deficiency of light, imperfect ventilation, impurities in domestic economy, unwholesome food and drinks, insufficient sewerage, want of cleanliness in the streets, and at the wharves and piers, and finally, to a general disregard of sanitary precautions, and the total absence of a regularly organized, adequate, and efficient sanitary police.

That the committee are correct in referring the general unhealthfulness of New York City, and the excessive mortality which prevails amongst its inhabitants, to its unfavourable sanitary condition, is very clearly shown by the fact that those wards where cleanliness and free ventilation abound, where the drainage and sewerage are ample, and where the houses are roomy, dry, well lighted, and not overcrowded with tenants, will compare favourably in respect to the amount and character of the sickness that occurs, and the ratio of mortality in reference to population, with any other city, while they stand in striking contrast with the less-favourably conditioned wards. In one of the best of the wards the ratio of mortality in 1827 was one only in every 69.68 of a population of 24,046; whilst in one of the most unhealthy wards, with a population of 13,486, the ratio of mortality, during the same year, was one in every 21.96.

The facts revealed in the report under consideration, and of the general accuracy of which there cannot exist a doubt, render it not at all surprising that so large a portion of the densely-built and thickly-inhabited wards of a city apparently so healthfully located as New York, and in possession, as it is, of all the material elements of prosperity, disease and death should make such alarming inroads upon its population, producing an amount of mortality scarcely equalled elsewhere, and which is constantly on the increase. The committee show, in the course of their investigation, that a very large percentage of the entire population are placed under circumstances, which, if they had been purposely designed to effect their wholesale destruction, could not have been more effective in their operation.

Although the same frightful condition, in a sanitary point of view, which is shown, in the report before us, to prevail in New York, cannot perhaps, with truth, be predicated of any other city in the Union, yet we are very certain that, in all of them, a properly conducted investigation would reveal the existence of many a source of pestilential emanations—many a plague spot where least suspected, and prove most clearly the absolute necessity of a well-appointed code of civic hygiene to insure the safety of all who dwell within their respective limits.

"Great cities are certainly the pride of nations, but they require a paternal control, and all Christian and civilized communities recognize the duty of exercising it. The difficulty has mainly been in devising the mode of using this power judiciously, so as not heedlessly to place restraints upon personal freedom. The aid of science must sometimes be invoked to strengthen and direct the hand of government, and to guide legislation in the duty of providing remedies for existing evils. A due regard for the public health of populous and growing cities calls loudly for the practical application of scientific principles, resulting from investigation into the causes of increased mortality, and of the best modes of prevention. Such investigations are constantly in progress, and it is not the part of wisdom to reject the counsels which they offer."

D. F. C.

ART. XXVI.—*A Lecture on Suspended Fœtal Animation, one of a Course delivered at the University Medical College, New York City, by* J. GAILLARD THOMAS, M. D., Lecturer on Obstetrics.

THIS is a most excellent lecture on a subject of deep interest to the obstetrician. It has been asserted by authorities of eminence, and we feel persuaded that the assertion is founded in truth, that, in every case of simply suspended animation in the new-born infant—that is to say, in every instance where there is no defect of development in any important organ, and where no vital part has received serious injury, either before or during birth—it is possible, by proper management on the part of the medical attendant, to induce respiration, and by so doing secure life and viability to the stillborn fœtus.

The lecture of Dr. Thomas presents a very fair exposition of the nature and causes of suspended animation in the fœtus, occurring during the progress or near the termination of labour, and of the proper course of proceedings to be resorted to in order to rouse the latent vitality of the stillborn infant into permanent activity. He handles the entire subject with particular ability, and in a manner admirably adapted to convey clear and distinct views in respect to it. The distinction of stillbirth into four forms, according to the difference in the cause or causes to which it is due in each case, is no doubt founded in strict truth, still we rather opine that it will not be so easy to make it out with any degree of certainty in actual practice.

Dr. T. divides the cases of stillbirth into those from asphyxia; those from syncope; those from cerebral congestion; and those from cerebral hemorrhage (apoplexy). And he thinks that each of these forms, with a little caution, judgment, and experience, may be distinguished from the others, so as to enable its appropriate treatment to be applied. We much fear, however, that this is true only to a very limited extent—but, at the same time, to an extent sufficient for all useful purposes.

Stillbirth from *asphyxia* is due, according to the lecturer, to non-oxygenation of the blood of the fœtus during the latter period of intra-uterine life, in consequence either of compression of the cord, of severe and protracted compression of the placenta from a spasmodic contraction of the uterus, as takes place especially under the influence of ergot; of hemorrhage from the partial or complete separation of the placenta previously to birth, and finally, of maternal toxæmia.

The condition of an infant born in a state of asphyxia is as follows: duskiness of face; purple and pouting lips; glassy eye; non-flaccid, unyielding limbs. There is either a very feeble or no pulsation of the heart, or an intermittent action, with entire absence of respiration.

According to the lecturer, when, in cases of stillbirth from asphyxia, there is no perceptible action of the heart, little or no hope of resuscitation is to be entertained. This is not our experience; we have known entire recovery to ensue in a very large number of instances where proper measures have been promptly resorted to, and sufficiently long persevered in, when not the slightest pulsation of the heart could be detected.

The measures directed to be pursued in these cases of asphyxia are particularly sound. They are mainly directed to the production of respiration, by removing any mucus accumulated about the fauces of the infant; by slapping it smartly upon the buttocks and chest; by blowing suddenly and violently in its face and mouth, and dashing water upon it with the hand. These failing, we are directed to expose the asphyxiated infant to a draught of cold air, and, while thus exposed, to put at once in practice Marshall Hall's plan of postural respiration. So soon as the action of the lungs is induced, circulation is to be promoted by upward friction of the limbs with the hand, without intermitting the respiratory process, and, when the stagnation of blood in the lungs is disappearing, by the use of the warm bath. An earlier employment of the bath should not be attempted, in consequence of its tendency then to add to the pulmonary

congestion, and to increase rather than to promote the removal of the existing state of asphyxia.

Stillbirth from *syncope* Dr. T. attributes to the fœtus being subjected during birth to causes by which a sudden cessation of the heart's action is produced, as compression of brain from overlapping of the sutures of the skull in its passage through the pelvis; loss of blood from rupture of the cord, or an arrest of the supply of blood by compression of the cord: the more unyielding coats of the arteries not allowing them to be so far compressed as to prevent entirely the flow of blood from the fœtus, while the less resisting coats of the veins permitting them to be so far compressed as to prevent a supply of blood being furnished to the fœtus from the placenta.

The propriety of referring the stillbirth, in cases in which the brain has been subjected during birth to inordinate pressure, to syncope is very questionable —it is doubted by Dr. T. himself. The condition of the fœtus is one rather of coma than of syncope.

The symptoms in this form of stillbirth are—paleness of surface, sometimes extreme; flaccidity of the limbs, and a sunken, cadaverous appearance of the face.

The first and most important measure under these circumstances is to establish respiration by the same means as in cases of syncope. The treatment in this and the last form of stillbirth, it is well remarked by the lecturer, is in fact exactly the same, with this addition, that in a child whose pale, bloodless, and wilted look indicates its anæmic condition, the cord should not be cut, and while an assistant promotes respiration, the funis is to be gently striped by the fingers in such a manner as to press all the blood which it contains slowly and steadily into the child's body.

Stillbirth from *cerebral congestion* is referred by Dr. T. to obstruction of the circulation through the vessels of the neck in cases of face presentation; to constriction of the same vessels by the cord, and to compression of the chest in cases of tardy delivery of the shoulders.

The appearances exhibited by the infant in this form of stillbirth are, a dark purple congested condition of the face; puffed and violet-coloured lips; protruding eyes, and general intumescence of the head.

The treatment in such cases is embodied, according to the lecturer, in the following three directions: 1st. Let blood flow freely from the severed cord, to the extent of an ounce or more, as the case may require; 2d. Establish artificial respiration; and 3d. Avoid the warm bath.

Stillbirth from *hemorrhage of the brain* is referred to intense or continued congestion of the encephalic vessels. It may be produced, also, by injury inflicted upon the fœtal head by its being forced, by violent uterine efforts, against some prominence in a deformed pelvis, or by the unskilful use of instruments.

The prognosis in this form of stillbirth is very unfavourable.

Cerebral hemorrhage is to be suspected when, to the symptoms of congestion, there is added a stupid, listless condition remaining after the establishment of respiration, or when an attack of convulsions supervenes, followed, in both cases, after a few hours, by death. In a few cases in which the infant survives, it may continue hemiplegic for many months, and ultimately recover.

The treatment is the same as in stillbirth from cerebral congestion.

Our high estimate of the sound practical sense which pervades the lecture before us, and a strong suspicion that the subject of which it treats is but imperfectly understood by a large portion of the profession, has induced us to deviate somewhat from our usual practice, by admitting a notice of it into a department of the Journal which is intended to be devoted to bibliographs of distinct treatises on subjects embraced in one or other of the branches of medical science. D. F. C.

ART. XXVII.—*Guy's Hospital Reports.* Edited by SAMUEL WILKS, M. D., and ALFRED POLAND. Third Series, Vol. V. London: John Churchill, 1859. 8vo. pp. 391.

THE present volume of this valuable publication contains twenty-two original communications, seventeen wood-cuts, and eleven lithographic plates. We shall lay before our readers a full analysis of its contents:—

I. *Contraction of Œsophagus from Corrosive Poison—Gastrotomy.* By J. COOPER FORSTER.

In the preceding volume of the *Reports*, an analysis of which is contained in the number of this journal for April last, is a communication giving the history of a case of epithelial cancer of the œsophagus, in which gastrotomy was performed by Mr. Forster. That case was not a very proper one for this operation, which, as was then stated, if ever advisable, could be so only in cases where the stricture of the œsophagus is owing to cicatrices, the effect of the action of hot liquids or of chemical agents; and the patient is not labouring under any other serious concomitant affection. The case recorded in the present communication is of this class; the patient, a child about four and a half years old, having swallowed, seventeen weeks before his admission to the hospital, some corrosive poison, supposed to be a solution of potash, or caustic alkali, used for bleaching and cleaning linen. The contraction of the œsophagus was consequent upon the injury induced by this escharotic, and with the exception of this lesion the child was perfectly healthy.

This patient entered the hospital on the 2d of February, 1859, and from that time until the 13th of March, the date of the operation, every effort was made to nourish him by fluids given by the mouth and by means of injections. For the two days preceding the operation he had not swallowed anything, was very exhausted, and appeared to be sinking fast. The operation performed was very similar to that which was done in the case related in the volume of the *Reports*, of which an analysis was given last April, and where the operation was fully described. It was attended, however, with rather more difficulty, principally from protrusion of coils of small intestine distended with flatus. By means of a canula introduced into the opening thus effected into the stomach, nutritive fluids were administered. Nutritive injections were likewise given *per anum*. The patient progressed most favourably until the morning of the fourth day, when, after feeding by the nurse, peritonitis of a fatal character supervened suddenly, and he died from the first shock of the inflammation. The autopsy showed that this peritonitis was owing to a giving way of the sutures, by which the stomach had been held in contact with the abdominal walls, for some of its contents were found within the abdomen lying between the liver and the diaphragm. The external wound in the abdomen had commenced to slough, and the stitches had loosened from their hold. It may be that the nurse, in introducing the tube, employed a little extra force, when the adhesions of the stomach to the parietes were necessarily very slight. It is a question, therefore, Mr. Forster says, whether he should not be induced another time to keep a tube constantly in the stomach for a week or ten days.

The unfortunate termination of this case points out the importance of waiting to open the stomach until adhesions have been formed between it and the walls of the abdomen. The necessity of this precaution was spoken of in the notice made of the case previously reported.

This communication is accompanied by a plate, showing the place of the incision in the abdominal walls.

II. *The Injuries and Diseases of the Nervous System.* By THOMAS BRYANT.

It is the intention of the author of this paper to illustrate, in a series of articles, the surgery of the different regions of the body, from cases that have occurred in Guy's Hospital. As he very truly says: "By giving the material from which facts and principles are deduced the author and his readers are placed upon a

par, and the latter are enabled to verify the truth of the former's opinions." Knowledge acquired in this method is attended with peculiar satisfaction, and is more relied upon and practically used than that imparted in any other.

The present paper includes injuries to the skull and spine, with their contents, briefly noticing, also, some of the diseases. The cases quoted therein have all been under the author's personal observation, and have taken place within the last five years in his own practice and that of his colleagues. They are, moreover, taken from his own notes. He says: "It will be my aim to illustrate the principal injuries which may take place, to give examples of each form, and to point out the symptoms by which each may be distinguished; to indicate by cases the complications which are generally found, and the pathological conditions and symptoms with which these complications are connected; and, lastly, the principles of treatment will receive attention, and the practice which should be employed in the various conditions illustrated by the quotation of examples." The whole source from which this material is drawn is thus tabulated—

	Cured.	Relieved.	Died.	Total.
Scalp wound	116	..	..	116
Concussion of the brain—simple	56	..	..	56
Concussion of the brain—complicated	24	..	2	26
Fractures of the vault of the skull	12	..	31	43
Fractures of the vault of the base	12	..	18	30
Apoplexy with injury	..	..	4	4
Concussion of the spine	22	..	..	22
Dislocation of the spine	..	..	6	6
Fracture and dislocation	..	3	15	18
Lateral curvature of the spine	..	5	..	5
Diseased spine—cervical	..	15	..	15
Diseased spine—dorsal and lumbar	..	19	..	19
Spinal abscess—cervical	..	1	..	1
Spinal abscess—psoas	..	22	3	25
Spinal abscess—lumbar	..	18	2	20
Spinal abscess—gluteal	..	1	1	2
Spinal paralysis	..	11	6	17
Total	242	95	88	425

The wounds of the scalp were all treated by sutures, strapping, pads of lint, or warm water dressing. Erysipelas, which has been regarded as a frequent complication of these wounds, occurred in but two instances. In these the application of flour to the head was the only local remedy; iron and stimulants were given internally. In one of the cases suppuration beneath the scalp took place, and a small piece of bone was subsequently exfoliated upon the fiftieth day.

The subject of concussion of the brain is considered under two heads:—

1st. *The simple form*, where an interruption only of the functions of the organ follows the injury.

2d. *The complicated form*, where the brain itself is injured, and associated with extravasation of blood, either upon or within its structure.

It must not be supposed, from this division of the subject, that Mr. Bryant is one of those who believe that an alteration of the functions of the brain does not necessarily involve any change of its structure. On the contrary, he does not doubt that in all cases of injury to the nervous centres, producing a suspension or alterations of their normal action, there is and must be some change in the structure of the brain or spinal cord, although that change may be overlooked by the naked eye. The *cases of paraplegia*, recorded by Dr. Gull in the preceding volume of the *Reports*, particularly Case XX., show how abnormal conditions of the nervous centres are rendered very evident by the microscope, when, without its aid, it would be impossible to detect them. *Complicated* cases of concussion, as they are called by Mr. Bryant, or those where the brain is evidently injured in its structure, are, in reality, cases of *contusion* of the cerebral structure, and there would be considerable advantage in classifying them, as Dupuytren has done, entirely apart from the others.

Of each form of concussion a number of interesting cases are given. In these, hemorrhage from the nose and ears and into the eyelids often occurred. When from the nose, the bleeding, as a rule, appears directly after the receipt of the injury and soon ceases; when from the ear it may continue for some few hours and then subside, but it is never followed by the clear serous discharge which is found in cases of fractured bone. When hemorrhage takes place into the eyelids it may be the result of a direct blow, but it is frequently produced by the concussion; the blood, however, will be confined to the eyelids, and will not appear as a subconjunctival ecchymosis as is seen in fracture of the base through the orbital plates.

In a number of instances of the simple form of concussion, after a partial return to consciousness, there was a relapse, and the patient returned to his insensible condition. This condition is denominated by Mr. Bryant a "relapsing unconsciousness," as adequately expressing the exact condition of affairs. It is so very generally, so universally, styled one of compression of the brain that Mr. Bryant might very well have given his reason for not making use of this expression. It would appear to be produced by the earliest effects of reaction, the increased quantity of blood pressing upon the brain and inducing a comatose or semi-comatose condition. In some of the cases the reaction was excessive, and complications evidently dependent upon inflammatory action manifested themselves. In these gray powder and Dover's powder, in three grain doses, were given three times a day.

Of the complicated form of concussion several cases are related in detail, with the condition of the brain as found at the *post-mortem* examination. In one "the brain was bruised all over, especially towards the anterior lobes, and upon its upper surface; and at its base the anterior and middle lobes were likewise ecchymosed. Blood also was extravasated at these injured spots. The fluid in ventricles was of a pinkish colour, and the parts around were ecchymosed." In another case there was, moreover, a layer of blood universally diffused over the whole surface of the brain. The action of *contre-coup*, in bruising the brain at a part corresponding to that portion of the head which was directly opposite to the one whereupon the blow was received, was very evident.

One of the cases is of great interest from its medico-legal bearings. A man, 56 years of age, when descending a ladder, fell from a height of thirty feet, striking his head in the descent against a projecting wall. He died on the second day with symptoms of violent contusion of the brain. At the autopsy, no fracture of the cranial bones was found; "at the base of the brain the arteries were in shape and quite rigid from disease, and in the substance of the hemispheres *were several points of extravasated blood about the size of peas.*" The heart was hypertrophied, and the left valves thickened. The kidneys were coarse and granular. Now, in this case, it may fairly be questioned, whether the man fell as a consequence of an apoplexy, or whether the extravasation of blood was the result of the fall. A number of doubtful cases of this kind are given in a communication from Dr. Wilks, contained in this volume of the *Reports*, which will presently be considered.

Fractures of the cranium are divided by Mr. Bryant into several classes as follows:—

A. Fractures of the skull unassociated with any encephalic complication.
B. Fractures complicated with simple concussion.
C. Fractures associated with extravasation of blood external to the dura mater.
D. Fractures complicated with extravasation of blood upon the surface of the brain.
E. Fractures associated with extravasation of blood within the cerebral structure.
F. Fractures complicated with depression of bone.
G. Fractures complicated with direct injury to the brain-structure.
H. Fractures at the base of the skull.

Of each of these classes several exemplifying cases are given; as many as twenty-six cases of these different forms of fractures being thus minutely de-

tailed. From their perusal the justness of the division adopted by Mr. Bryant is apparent, for *injuries to the skull are important just so far as they involve the cranial contents.*

Though the effect of the *contre-coup* in producing contusion of the brain was very visible in these cases, in none of them did it produce fracture of the skull. Indeed, a fracture of the skull by *contre-coup* never occurred, according to Mr. Bryant, and we are strongly inclined to agree with him; in the first place, because we find the arguments of M. Aran unanswerable;[1] and, in the second, because we have never seen one, though we have examined several collections for this purpose. Such a fracture is only a continuation of a direct fracture, of a fracture at the point struck.

As indicative of fracture at the base of the skull, hemorrhage from the nose, from the ear, and into the subconjunctival cellular tissue, are symptoms whose significance and value have been much discussed. Of Mr. Bryant's cases, in eight there was hemorrhage from the ears; in all this was followed by a discharge of a serous fluid, and in seven cases associated with paralysis of the facial nerve upon the same side. Two of these cases proved fatal, and in them both the fracture passed through the petrous bone. In three cases there was epistaxis; in one of these the fracture extended across the ethmoid bone; in one, the frontal sinuses were full of blood and fractured; and in the third the tympanum was found full of blood, and the membrana tympani perfect, and, upon careful examination, the lateral sinus of the brain was found to have been lacerated. Subconjunctival hemorrhage occurred in four of the fatal cases, and in one and all the line of fracture extended through the orbital plate.

The source of the serum, or of the colourless liquid, which flows from the ear, and at times from the nose, in cases of fracture of the base of the skull, and which is one of the most important signs of such an injury, is a question of considerable interest. M. Laugier, a surgeon of Paris, was the first to observe this, in 1835, and he, remarking that first blood came away, and afterwards a colourless liquid, supposed that it was the serum of the blood, flowing away after its coagulation. The chemical analysis of this fluid shows, however, that it does not come from the blood or the bloodvessels, for it contains very little albumen, or none at all. The generally received opinion now is, that it comes from the cephalo-rachidian liquid, to which it is similar in chemical analysis, and where the anatomical lesions have been minutely noted, they have been found to be such as to permit this liquid to escape.

In regard to the use of the trephine, some valuable conclusions may fairly be drawn from the consideration of these cases. Simple fractures of the skull, although associated with depressed bone, if unattended with marked symptoms of compression, are to be left alone; as a rule they terminate successfully. When symptoms of compression arise, if the injury be *local*, the bone, Mr. Bryant says, should be elevated. Here we would add that the surgeon should be exceedingly careful that the symptoms are of such gravity as to absolutely require the application of the trephine. In using it, he converts a simple fracture into a compound; he opens, very probably, a collection of blood in the cranial cavity and exposes it to the air, and if the dura mater be torn he opens the arachnoid; such complications expose to very great dangers, as well as does the compression of the brain. When compression of the brain follows a *general injury*, the operation of trephining is useless, if not injurious. Compound fractures of the skull, attended with depression and comminution of the bones, are to be treated by the removal of the loose portions; and if symptoms of compression exist, and the bones cannot be removed without the use of the trephine, that instrument is to be employed, although with great care, as severe injuries to the cranium may recover, so long as the membranes are entire and the cerebral hemispheres uninjured. When, Mr. Bryant says, compression of the brain follows as a secondary result of a *local injury* over the course of the meningeal artery, that is, after an interval of time, when reaction has been established, although no depressed bone may be present, it is probably produced by a rupture of one of the arterial branches; the operation of trephining may then be

[1] See his Memoir in the Archives Générales de Médecine for 1844.

performed with a chance of success, although it is rare to find a very local extravasation, the blood generally passing downwards towards the base, where the operator cannot reach. One of the cases, the XIX., is the only one in which the patient recovered after such an operation. In this, after the removal of three small pieces of bone—the operation was performed six hours after the receipt of the injury—a small clot of blood was exposed and removed, lying external to the dura mater, which was uninjured. The case was one remarkably suited to the operation, which is one upon which the surgeon cannot place any great hope, in the vast majority of cases, for all the operations practised for the relief of traumatic effusions, in Paris, for a space of more than fifteen years, have, without a single exception, been followed by the death of the patient.

There is nothing calling for special notice in what Mr. Bryant has to say of concussion of the spine. As is seen by the table, all of the cases, twenty-two in number, recovered. In the simplest form there is but slight paralysis and loss of sensation of that portion of the body supplied with nerves from the injured centres; in more severe cases there is some retention of urine, from loss of voluntary power over the muscles which regulate micturition; and in still more perfect examples of concussion the paralysis and anæsthesia of the body may be entire, and associated with absolute paralysis of the bladder and all its muscles, allowing the urine to flow from its cavity as secreted. This condition of the bladder, be it remarked, must not be confounded with the involuntary discharge of urine from an over-distended viscus, which is witnessed in the simpler class of cases.

The very grave nature of the next class of injuries in Mr. Bryant's table, is evident from a glance at the numbers attached. Of the twenty-four examples of fracture and dislocation of the spine therein recorded, only three are seen to have escaped with their lives, and these are marked as only "relieved," that is to say, the functions of the cord were not perfectly restored. Dislocation of the spinal vertebra is generally taught to be an accident exceedingly rare; but according to Mr. Bryant's experience, this opinion is erroneous. Of these 24 cases, 6 were cases of pure dislocation of the spine, 3 of fracture, and 8 of fracture and dislocation combined; and the fact in these seventeen cases was verified by post-mortem examination. In the remaining seven cases a fracture was diagnosed, but not proved. As might be supposed, dislocation is much more common in the cervical than in the dorsal region; and in both the upper vertebræ, as a rule, is the one thrown forward. Of these twenty-four cases, the cervical region was affected in ten; the dorsal in fourteen, in one of these three of the upper lumbar vertebræ being also fractured. In reviewing the ten examples of injury to the cervical region, it appears that in two only was life prolonged beyond forty-eight hours after the accident; in one of dislocation between the fourth and fifth vertebræ, with disorganization of the cord, the patient survived but thirteen hours; five lived only thirty-six, and two forty-eight hours. In all these the disorganization of the cord had taken place opposite the fifth or sixth vertebra. In two examples the injury to the spine and cord corresponded to the seventh cervical; one of these lived seventy-two hours, and the second seven days. In all of these cases but two the respiration from the beginning was diaphragmatic, and in these it became so at the expiration of forty-eight hours in one instance, and of ninety-six in the other. Of the eleven fatal cases of fracture and dislocation of the dorsal vertebra, three died on the tenth day, and eight within eighteen days; the other three died, from a gradual sinking and from bed-sores, on the 90th, the 134th, and the 232d days, respectively. In the three cases that recovered, the injury was about the three lower dorsal vertebræ.

Of the cases reported in detail by Mr. Bryant, the two most interesting are mentioned in the analysis given in this journal of the last volume of the *Reports*, having been published by Dr. Gull in his *Cases of Paraplegia*. They are the cases marked as the XXIII. and the XXIV. in that communication.

Mr. Bryant says but very little of the remaining affections of the spine, mentioned in his table, a few lines only being devoted to them, and in these there is nothing calling for notice. Readers anxious for more information upon them, are referred by him "to the standard works upon the subject."

III. *Cases of Ruptured Uterus during Parturition.* By J. BRAXTON HICKS, M. D., Lond., F. L. S., &c.

This communication contains the history of three cases of rupture of the uterus during parturition; in one of which the cervix was lacerated; in another there was a longitudinal rupture, about seven inches in length, extending from just above the os; and in the third the rent was such as to have allowed the child to escape into the peritoneal cavity, where it was found. In other words, there is a case of each one of the three classes into which rupture of the uterus may very properly be divided.

With regard to the frequency of rupture of the uterus, in 12,400 cases of midwifery, collected by Dr. Hicks from private sources, all from thoroughly good practitioners, there occurred only two cases about which there could be no doubt, and two in which there was uncertainty, as no post-mortem examination was made. Since the last septennial report of the Lying-in Charity attached to Guy's Hospital, 8,982 cases were attended by the pupils, and in these only two certain cases occurred, and none suspected. In the three septennial reports already published, there occurred, in 22,498 cases, five certain cases, and five uncertain. This gives a total of 43,800 cases, with nine certain cases of rupture, or 1 in 4,875; or if the uncertain cases be included, 1 in 2,736. Of these all the undoubted cases died; and of the uncertain, six out of seven. In only one did the contents of the uterus escape into the peritoneal cavity.

IV. *Cases of Morbus Addisonii, Melanœmia, Anœmia Idiopathia, Leucocythemia Splenica et Lymphatica.* By SAMUEL WILKS, M. D.

This communication, as its title indicates, is upon several obscure forms of disease, which are, at the present time, exciting much attention throughout the whole medical world. The first case it contains is one of the affection known as *morbus Addisonii*, or disease of the supra-renal capsules. A man, 32 years of age, in an extremely debilitated condition, and with his skin of a dark colour, resembling that of a mulatto, died rather suddenly in the hospital, three days after admission. He had previously, for more than two years, been under observation, and during all this time his colour was the same. At the autopsy all the organs were found healthy or nearly so, with the exception of the supra-renal capsules, both of which were quite destroyed in structure by adventitious deposit. This deposit consisted of an albuminous-cretaceous material, which the microscope showed to be almost structureless; it was an amorphous substance mixed with fatty and cretaceous granules. The colouring matter in the skin was situated beneath the epidermis, in the rete mucosum, following the course of the papillæ, thus exactly resembling the integument of a negro. In a note to this communication, and also in the *addenda*, seven other well-marked cases of a similar character are said to have come under the notice of the author, since the above case was recorded.

In some remarks of considerable length appended to the history of this case of Addison's disease, Dr. Wilks attempts to answer the numerous and weighty objections that have been brought forward against the views and conclusions of Dr. Addison. The most important arguments and facts that have been published in relation to the existence or non-existence of this affection, have been given in the quarterly summaries of the improvements and discoveries in the medical sciences in this journal, and there is very little calling for present notice in what Dr. Wilks here says. It has been shown that supra-renal disease has been found, when there was no discoloration of the skin, and also that pigmentary discoloration constantly occurs without any morbid affection of these organs. In answer to the first of these objections, Dr. Wilks declares that the skin does not become discoloured until the disease has existed a long time, the correctness of which statement additional facts are required in order to prove; and the second objection he does not answer at all clearly—to our mind it remains in all its force. When, moreover, it is taken into consideration that in the lower animals, when the capsules are extirpated without great injury to the solar plexus, the animal continues to live as before; and that, in men, the capsules have been found entirely wanting, as if congenitally deficient, without discoloration of the skin and marked debility during life, we see strong reasons for re-

fusing to believe that there is such a connection as that of cause and effect, between disease of the supra-renal capsules and the symptoms of the fatal malady known by the name of *morbus Addisonii*.

The next article in this communication is entitled *Melanæmia and Discoloration of the Tissues arising from Intermittent Fever*. The observations of a number of physicians have recently shown that at times, in intermittent fever, the blood undergoes a disintegration in the spleen, and the hæmatine from the broken-up corpuscles circulates through the system until it becomes impacted in the capillaries of the various organs of the body, causing them to assume a dark colour. The reasons for supposing that the pigment is formed in the spleen, are, that in the spleen of man and other animals cellular elements inclosing blood-globules or pigmentary matters are often met with, and that pigment is never found in other organs, except also present in the liver and the spleen. The symptoms of the affection are the condition of the blood, resembling that of chlorosis, and the troubles caused in the liver, brain, and other organs, by the obstructions formed by the deposit of the colouring matter. Dr. Wilks has nothing new on the subject of this disease, his article being made up of extracts from the chapter entitled: "The Pigment Liver, or Melanæmic Liver," in Frerichs' lately published work, and of the history of two cases from the second volume of Dr. Bright's *Medical Reports*.

Following this article is the history of a case similar to those contained in a communication entitled: "*Idiopathic Fatty Degeneration, with Remarks on Arcus Senilis*," in vol. III. of this series of the Reports, of which an analysis was given in this Journal for October, 1859. The same affection is here styled simple or idiopathic anæmia. At the autopsy in this case, no cause could be detected in any of the organs for the great anæmia under which the patient had laboured; there was some fatty degeneration in the heart, as might be expected where an anæmic condition had existed for three years. No mention is made of the condition of the intestinal canal, which might have revealed the cause of the anæmia, for the patient had been subject to diarrhœa during the whole time of her illness.

The remainder of this communication is occupied by the reports of six cases of anæmia accompanying enlargement of the lymphatic glands and the spleen, or *anæmia lymphatica*, and *leucocythemia splenica;* two affections Dr. Wilks believes to be distinct. A full history of the most interesting of these cases, with an abstract of others, and a short account of Dr. Wilks' views thereupon, is contained in the October number of this Journal, 1859.

This communication is accompanied by two plates, one representing the face of the man affected with melasma supra-renale; the other, the diseased supra-renal capsule in its position upon the kidney.

V. *Sanguineous Meningeal Effusion (Apoplexy); Spontaneous and from Injury.* By Samuel Wilks, M. D.

The difficulty, or even the impossibility, of determining, in certain cases, whether an effusion of blood found on the surface of the brain, or in the substance of the hemispheres, is owing to an injury received, or to disease, was very briefly referred to in connection with one of the cases contained in the communication of Mr. Bryant. Of course, in the large majority of cases, both of injury or disease, there is no difficulty in coming to a conclusion as to the nature of the case, for in the former the external parts are affected, while in the latter the mischief is within the cranium. But at times, and in meningeal effusions particularly, both causes mentioned may produce the same effect, and if the patient has been known to have received a fall and to have struck his head, the case must prove a most perplexing one.

Dr. Wilks relates some cases where the effusion was known to be spontaneous; others where a blow had been received; and again, others where the cause was dubious. In all of these cases the immediate cause of death was pressure upon the brain from meningeal apoplexy; the post-mortem appearances being similar in all. Of the spontaneous cases, the patients, who had been some time in the hospital, had Bright's diseases in two instances, and idiopathic anæmia in the third. The two doubtful cases were comatose when admitted;

one had fallen in the street, and was said to have uttered an exclamation referring to his head before he fell; the other, after previously feeling giddy, had fallen, and struck her head against the door.

It is important to be well aware of the occurrence of cases of this kind, not so much on account of the patient, as on account of their medico-legal relations.

VI. *Cases of Poisoning.*

This communication contains an account of the most interesting fatal cases of poisoning that have occurred in the hospital since the publication of the last volume of *Reports*. One was by Burnett's disinfecting fluid (chloride of zinc), one by brandy, and three by sulphuric acid.

The disinfecting fluid[1] was swallowed by a woman, 40 years of age, in mistake for a glass of gin. It remained on the stomach only about ten minutes, when it was vomited up again. A burning sensation was felt in the throat and chest for two or three days, when it disappeared, leaving nothing but an inability of the stomach to retain food. Death ensued at the expiration of fourteen weeks; apparently from simple prostration, due to want of nourishment. At the autopsy, the stomach was found so much contracted as to contain only a quarter of a pint of fluid, and completely perforated in two places by ulcers, one near the cardiac, the other near the pyloric orifice. There was no decided peritonitis, but the whole of the serous membrane had a slightly greasy feel when touched, as if there were some exudation on the surface.

In the case of poisoning by brandy, the patient, a man 27 years of age, had drank his fill from a gutter, into which about a gallon had been poured from a broken bottle. Taken to a station-house in a state of intoxication, the stomach-pump was applied, and emetics administered. The stomach-pump was again applied some hours afterwards on his admission to the hospital, but nothing was removed. He was then cold, collapsed, and unable to swallow. He died twelve hours after admission, and twenty-two after the debauch. At the autopsy, twenty-four hours after death, all the organs were found much congested but structurally healthy; a small quantity of spirit was obtained from the contents of the stomach; there was no odour about the brain, and no spirit could be obtained by distillation.

The soap-lees were swallowed by a child 18 months old. There was some difficulty of breathing, but the most marked symptom was intense heat of skin. Death followed in twelve hours. At the examination of the body, the larynx was found to be almost closed, at its top, by the greatly swollen epiglottis, and the mucous membrane from the mouth to the duodenum, particularly at the lower extremity of the œsophagus, was changed in character by the alkali, though nowhere destroyed.

In the cases where sulphuric acid was swallowed, death resulted from the destruction of the mucous membranes that came in contact with the poison, and also from the sero-albuminous exudation into the submucous tissue. One patient lived four days, another eleven, and the third, a child but 17 months old, died in seven hours, although tracheotomy was performed to relieve the suffocation caused by the inflammatory œdema of the submucous tissue about the top of the larynx.

Two cases are added to these to illustrate the local effects on the mouth and throat of sulphuric and nitric acids. In these, glycerine was applied to the parts, and a mixture of carbonate of magnesia, tincture of opium, and mucilage, was given internally. In one instance, that where sulphuric acid was taken, the mucous membrane of the mouth and fauces peeled off, and was afterwards restored. The ulterior effects of caustic poisons in causing constriction of the œsophagus, are seen in the case of gastrotomy previously described.

VII. *Description of some new Wax Models, illustrating several Cases of Elephantiasis, Diseases of the Nails, etc. etc.*

This communication, though a very interesting one, is, of course, more particularly to be appreciated and used by those who are able to study the models therein described.

[1] According to the U. S. Dispensatory, each ounce of Burnett's solution contains 200 grains of chloride of zinc.

VIII. *Malposition of the Abdominal Viscera in relation to the Causes and Diagnosis of Disease.* By S. O. Habershon, M. D.

This communication contains the history of a case in which there was complete transposition of the thoracic and abdominal viscera. The patient was a woman, 48 years of age, who was dying when brought to the hospital, and the condition of things was detected only at the post-mortem examination.

It contains, in addition, a minute account of changes in position undergone at times by the different portions of the intestine in the abdomen, showing the uncertainty of resting solely on their normal site as our guide in diagnosis. These changes in position are caused by distension, by rotation, by the effect of old adhesions, and by congenital arrangement.

The communication is an interesting and instructive one, so far as it goes, but the title led us to look for something more than merely this account of the changes in position of portions of the intestinal tube, which forms by no means all the "abdominal viscera."

A plate is attached representing the position of the viscera in the case above mentioned.

IX. *Gunshot Wound in the Loins; Fracture of the Lamina of the Vertebra; Perforation of the descending Colon; Fecal Abscess; large white Kidneys; Albuminuria; Death nearly Four Years after the Injury.* By S. O. Habershon, M. D.

The patient whose very curious case is here described was a soldier, 18 years of age, who was wounded in the trenches before Sebastopol. A ball, weighing more than one and a quarter ounces, passed through the spinal canal, breaking the lamina of the third and fourth lumbar vertebræ. The point where it entered was below the termination of the cord; and it is probable that neither the membrane surrounding the terminal nerves before their exit, nor the large veins upon the vertebra, were injured. Pain for a short time down the thighs was the only symptoms which could be referred to any injury to the spinal nerves. Three days after the wound the ball passed *per anum*.

The renal disease, which was the cause of the man's death, is attributed, by Dr. Habershon, to the contiguity of the kidneys with the inflamed tissues injured by the ball. He has several times observed such disease to arise after chronic dysentery has continued some time.

This communication is accompanied by a plate, exhibiting the gunshot wound in the loins extending into the colon.

X. *Cases of Pyœmia.* By S. O. Habershon, M. D.

These cases are two in number. The first is that of a woman, 34 years of age, whose condition for some time had been one of extreme destitution. She was admitted as labouring under a severe ague, having every day rigors, followed by heat and sweating. Large blebs were developed on the skin, especially on the hands and feet. She became more and more prostrate, notwithstanding the treatment, which was throughout stimulating, until her death, which took place on the eleventh day, or about twenty-five days from the commencement of the disease. There was nothing to be particularly noticed in the state of any of the organs as examined after death.

"Pyæmia," Dr. Habershon says, "is the name of a condition with generally well-marked symptoms; but it is probable that in other degrees of severity it comprehends many cases of so-called rheumatism, and probably, also, such as that narrated." We think this case would be generally regarded as one of acute pemphigus rather than one of pyæmia, no matter of what degree of severity. Similar cases are thus described by Cazenave and Schedel, and several other writers on diseases of the skin.[1]

The other case is that of a young man, 19 years of age, who, while suffering from gonorrhœa—how long the running had existed is not stated—had slight rheumatism of the knees and ankles, and lobular pneumonia at the base of the right lung. A few days afterwards, when considered as convalescent, he got up to go to the night-chair, and, after the bowels had acted, complained of faint-

[1] See the work of Cazenave and Schedel, 4th edit., page 202.

ness; carried to his bed, in half an hour he had ceased to breathe. At the autopsy it was found that there had been phlebitis of the prostatic veins, which contained a well-marked ante-mortem clot that extended through the iliacs, the vena cava, the right side of the heart, and the pulmonary arteries throughout the lungs.

This case, Dr. Habershon says, tends to confirm the ideas of those physicians who affirm that the symptoms resembling rheumatism, produced by gonorrhœa, arise from the absorption of noxious materials leading to change in the molecular or chemical states of the blood; in fact, producing pyæmia.

XI. *Cases Illustrative of the Treatment of Rheumatic Fever.* By G. WHITLEY, M. D.

The object of this paper is to ascertain, as far as possible, by an examination of the cases of rheumatic fever treated in the wards of Guy's Hospital by the different physicians during the last three or four years, what mode of treatment appears most frequently and most rapidly successful. For this purpose the cases of acute fibrous rheumatism have been selected, and twenty-three such are briefly reported.

In fifteen of these cases salts of potash were given, either at first or after the failure of other means. In no instance did they fail to effect a cure, which, even in severe cases, was sometimes attained very rapidly. Some recent acute cases treated with lemon-juice made a very rapid cure, but in many instances this remedy proved ineffectual.

From what is known of the nature of rheumatic fever the alkaline treatment would certainly seem to promise most success. The chief indications in the treatment are, to prevent the formation of an undue amount of lithic acid, or to favour its conversion into urea, and to facilitate the elimination of the fibrin present in excess in the blood. Alkalies not only act as solvents for lithic acid, but they also dissolve fibrin, and, in addition, their neutral salts, especially those of potash, are diuretic.

XII. *Lesions of the Nervous System Producing Diabetes.* By F. W. PAVY, M. D.

In a bibliographical notice contained in the number of this journal for April last will be found a very full analysis of a most important paper on the sugar-forming function of the liver, published by Dr. Pavy in the last volume of the *Reports*. The experimental investigations therein detailed controvert the views entertained by physiologists with respect to the production of sugar by the liver during life. They show conclusively that in the healthy organ, during life, there is a substance which, in its chemical bearings, is exceedingly like dextrine, and prone to most rapid transformation into sugar when in contact with complex nitrogenized animal materials. In the liver after death this transformation takes place, and with such astonishing rapidity that this organ has been erroneously supposed to be strongly charged with a saccharine principle when the animal is alive. The fact is, however, as Dr. Pavy has proved beyond a doubt, that naturally, during life, there exists a force, or condition, which overcomes this strong chemical tendency to a saccharine metamorphosis; and the hepatine, as he terms this peculiar substance, does not undergo this conversion. In pursuing his researches during the past year, Dr. Pavy has been led to follow out new paths of inquiry; and, in this present communication, he proposes to mention the facts that have been brought to light in the course of his experimental investigations concerning the disturbances of the nervous system that produce saccharine urine.

The fact from which Dr. Pavy now starts, is this: When, after the pithing of an animal, artificial respiration is resorted to, so as to allow the heart's action to continue and the circulation of the blood to go on, then the sugar formed as a *post-mortem* effect in the liver is carried from it and distributed through the system to be eliminated with the urine. When, however, the circulation is not kept up no sugar, under these circumstances, is to be discovered in the system. An experiment in proof of this was mentioned in his last paper; and two are detailed in this, in one the lower part of the medulla oblongata was divided, in the other the central portion, which show that when artificial respiration is

kept up the urine becomes strongly saccharine, and that this results from the ordinary post-mortem change in the liver furnishing sugar, which is carried by the circulation to the kidneys, thence to be discharged by their action. This fact, therefore, must be considered as established, namely, that after the destruction of the medulla oblongata the cause preventing the transformation of hepatine into sugar ceases to act.

When the spinal cord was divided, and in some of the experiments the division was made as high as between the second and third vertebræ, when, of course, artificial respiration was required to maintain the circulation, diabetes was never produced. With regard to the brain, when the experiments are attended with almost insuperable difficulties, it is scarcely allowable to speak positively, but it is almost a matter of equal certainty that its functions may be completely destroyed without the urine becoming saccharine. It is then only when the medulla oblongata ceases to act, from the death of the animal, or from its destruction, that the hepatine is transformed into sugar, or, in other words, that the liver loses a force or a condition that checks the chemical tendencies of hepatine.

The channel by which the medulla oblongata transmits this influence downwards was then endeavoured to be established. The spinal cord cannot constitute the passage for the transit of this force, because, as just said, after its division the liver remains in an ex-saccharine state. Neither could it be the pneumogastrics, for these nerves have been divided over and over again without the production of diabetes. Dr. Pavy divided, also, in the same animal, both the spinal cord in the cervical region and the two pneumogastrics, and no diabetes resulted. When everything belonging to the nervous system passing through the neck was divided, which was done by decapitating the animal, and artificial respiration was employed to keep up the circulation, the urine was strongly saccharine. The channel of communication, therefore, must be through the sympathetic nerves. Section of the carotid sympathetic has frequently been performed, and no one has ever spoken of it as leading to the production of diabetes. There is, however, another part of the sympathetic system running through the neck, namely, the filaments accompanying the vertebral artery. When these were divided Dr. Pavy obtained a most strongly-marked diabetes. At first Dr. Pavy supposed that this was owing to the influence of the medulla oblongata being thus cut off from the liver, but certain phenomena were observed in subsequent experiments that tend to show that there is something yet to be discovered beyond this. Our knowledge of the sympathetic nerve is so imperfect, and its connections are so numerous, that it is very difficult to understand the reason of all that is observed in experimenting upon it.

Commencing then at this point in the vertebral canal in the lower part of the neck, Dr. Pavy followed up the sympathetic nerve, proceeding upwards towards the head, and downwards through the chest towards the abdomen. A great number of experiments were performed in pursuing this investigation, the animal operated upon being generally a healthy dog, kept upon an animal diet and taken at a period of full digestion. Each of the experiments is minutely detailed, as are all those that were performed at each step of this investigation. Their results, which, notwithstanding the great interest attached to every point in these researches, is all that can be given in this analysis, will be briefly stated.

When the sympathetic filaments, running up on each side from the superior thoracic ganglion to the vertebral canal, were torn through, the urine became saccharine. Wishing to see the effect of dividing the filaments in the vertebral canal, and believing that this could not be done without severing also the arteries which he thought would produce fatal hemorrhage, Dr. Pavy tied these arteries before their entrance into the vertebral foramina, and also the two carotids on account of the free anastomosis existing at the base of the brain between these two pairs of vessels. The ligature of these vessels did not produce diabetes; and when, under these circumstances, the division of the two vertebral canals was effected diabetes resulted. Having found, in the course of these experiments, that no apprehension of death need be entertained from loss of blood if the section were made without any deligation of the arteries, the

vertebral canals only were operated upon, and to Dr. Pavy's astonishment the urine did not become saccharine. The inclusion of the carotids in the experiments was, therefore, a necessary condition for the production of diabetes. He sought, then, to discover what could be the influence of the ligature of the carotids in determining this effect. As diabetes was not produced when they were carefully tied together with the vertebrals it could not be from the simple obstruction made to the passage of blood; it must have been owing to the destruction of the sympathetic filaments ramifying upon the exterior of these vessels. At the upper part of the neck there is quite a large oval-shaped ganglion that sends one branch to the pneumogastric and numerous filaments to the carotid artery. When this was removed an intensely diabetic effect was produced; of all the operations performed, this was the most certain and speedy in producing diabetes. It is, therefore, reasonable to suppose that the destruction of its carotid branches, conjoined with the destruction of the sympathetic in the vertebral canal, is the cause of the result obtained. The destruction of either singly is not sufficient to produce the effect which the destruction of the two would seem to be the cause of inducing. When the carotids were tied, and the anterior and posterior surfaces of the transverse processes of the atlas were scraped bare so as to destroy all that laid in connection with the vertebral foramen, the urine gave a decidedly strong reaction of the presence of sugar.

Upon the sympathetic in the chest a number of operations were performed. In some of these experiments the strongest diabetes was noticed; in others, from some cause, which it is not easy to discover, only traces of sugar could be found in the urine; and in others, there was none at all. To ascertain the effect of dividing the nerves immediately connected with the liver —those contained in the lesser omentum—two experiments were made. In both of them a considerable amount of hemorrhage resulted. Neither of them was attended with the slightest discoverable diabetic effect.

From this series of experiments the evidence is most conclusive that lesions of certain parts of the sympathetic system produce strongly-marked diabetes. What the chain of phenomena is, by which such an effect is determined, constitutes a most important problem, one, too, most difficult of solution; but we may hope for much when it is being investigated by so able and industrious a physiologist as Dr. Pavy has more than once proved himself to be.

This communication is accompanied by a plate showing the cervical and thoracic sympathetic of the dog.

XIII. *Contributions to the Practical Surgery of New Growths or Tumours.* Series III., *Cysts.* By J. BIRKETT.

In this communication those new growths dependent upon the formation of cysts are illustrated. Those developed on the integument of the whole external surface of the body, and also those in so much of the mucous membranes of the oral orifice and the female genital organs as are within sight, are included. Though it has a certain value, this contribution, from the very nature of the subject, possesses by no means the same interest as the others of this same series, published in the two preceding volumes of the *Reports*. The pathology and treatment of the cysts that form in relation with the glands which secrete sebaceous matter, and with those which secrete mucus, are well understood. Mr. Birkett relates twenty-one cases of the former, and fourteen of the latter.

Two plates are attached to this communication, one to illustrate a case of labial cyst; the other one of sublingual cyst, or ranula.

XIV. *Cases of Aneurism of the Cerebral Vessels.* By WILLIAM GULL, M. D.

According to Dr. Gull, aneurism of the cerebral vessels is not so extremely rare a disease as has been supposed. It is only within the last ten years that such cases have been looked for, and already quite a number of cases have been reported. It is, moreover, an affection which, even when looked for, is very likely to escape detection. When death occurs from rupture of the sac, recent coagula may so imbed and conceal it that unless very carefully looked for it will not be found, for the sac is often small and thin and transparent, except at the point of rupture. Again, when death has taken place from changes around the aneurism, as by pressure or softening, the sac itself may present such appear-

ances that unless a minute dissection be made of it, its true nature may not be discovered.

Of sixty-two cases of inter-cranial aneurism, collected by Dr. Gull, the seat of the disease was as follows:—

Vertebrals and branches.	Vertebrals	4
	Basilar	20
	Small vessels in substance of pons	1
	Posterior cerebral	3
		28
Carotids and branches.	Internal carotids by sella turcica	8
	Middle cerebral	15
	Anterior cerebral	6
	Anterior communicating	1
	Posterior communicating	4
		34

Of 58 of these cases, where the sex is given, 35 were males, and 23 females. The age is also given in fifty-eight cases, and they are distributed as follows:—

Under 25 years	12
25 to 40 "	13
40 to 60 "	29
Over 60 "	4
	58

Tables are added giving the synopsis of the symptoms of aneurism of the basilar artery, of those of aneurism of the middle cerebral artery, of those of aneurism of the anterior cerebral artery in the substance of the hemisphere, and of those of aneurism of the arteries of the circle of Willis. The detailed histories of six cases of inter-cranial aneurism are also contained in this communication.

There do not appear to be any symptoms or order of symptoms, upon which a diagnosis of cerebral aneurism can be made. The liability of the cerebral vessels to this lesion must, however, always enter into our calculations in the general diagnosis of tumours, especially when there is evidence of local pressure on parts about the base of the brain.

This communication is illustrated by two wood-cuts and one plate.

XV. *On Retroversion of the Aortic Valves from Disease in the Sinuses of Valsalva.* By William Gull, M. D.

The wonderful simplicity and perfection of the action of the sinuses of Valsalva for adapting the aortic valves to prevent regurgitation during the diastole of the ventricle are well known to physiologists. The case here reported by Dr. Gull draws attention to an accident that may occur when these sinuses become rigid from ætheromatous changes. Some of the conditions by which retroversion of the aortic valves may be produced have already been referred to, but the direct influence of the want of elasticity in the sinuses has never been noticed. The case is thus related:—

John H——, æt. 39, a day labourer in the docks, always had good health until ten weeks before admission into Guy's Hospital. His symptoms—breathlessness, orthopnœa, palpitation, and cough—came on suddenly. He never had rheumatism. The distress of breathing continued, and was attended with so much pain across the chest, that an aneurism of the aorta was suspected. The pain was, however, probably attributable to sudden and continued distension of the left ventricle. The stethoscopic symptoms were only those of aortic regurgitation. He remained unrelieved, and died after being five weeks in the hospital.

At the autopsy the sinuses of Valsalva were found extremely ætheromatous, and the aortic valve corresponding to one of these was retroverted.

This communication is illustrated by a plate.

XVI. *On Destructive Changes in the Lung from Diseases in the Mediastinum Invading or Compressing the Pneumogastric Nerves and Pulmonary Plexus.* By WILLIAM GULL, M. D.

Physiologists have proved by experiments, that, when in animals both the pneumogastric nerves are divided, the lungs undergo destructive changes. The effect of dividing only one of these nerves is not so well determined, some physiologists affirming that the corresponding lung becomes inflamed, while others, and they are the great majority, declare that no obvious effects are produced. Experiments, however, can only be made upon these nerves in the neck, and as they communicate freely in the pulmonary plexus, it is probable that one nerve can maintain the normal integrity of this plexus, and morbid effects will not result from injury to the other. When, however, the nerves and their plexus are injured by disease lower down about the root of the lung the results are different, because the branches going to either lung may be individually implicated, and consequently the corresponding lung be directly deprived of its nervous supply. This is illustrated by the three cases detailed in this paper. In one of these an aneurism of the left side of the arch of the aorta pressed upon the left pneumogastric nerve and the upper part of the pulmonary plexus, and there was sloughing pneumonia of the left lung. In the second, cancer of the œsophagus invaded the trunk of the right pneumogastric nerve and the branches of the pulmonary plexus behind the right bronchus; the result was pneumonic consolidation and commencing gangrene of the right lung. In the third, a fibrous thickening of the tissue in the mediastinum implicated the right pneumogastric nerve and the branches of the pulmonary plexus; the pulmonary tissue in the right side was consolidated, infiltrated with pus, and sloughing, and the bronchial tubes were much dilated and full of muco-purulent secretion.

The mode in which injury to the pneumogastric nerve produces such disastrous effects upon the lung, is by paralysis of the bronchi and air vessels. The bronchial tubes become unable to empty themselves of their contents, a muco-purulent secretion accumulates, concomitant exudation into the air-cells takes place, and at length disintegration of the tissue.

XVII. *On Factitious Urticaria.* By WILLIAM GULL, M. D.

In this communication, attention is directed to a class of cases in which wheals are produced upon the skin when rubbed by the dress or otherwise, or when stretched by the use of the muscles in violent exertion. Though this susceptibility of the skin is common, in a greater or less degree, to all persons, in some it is so extreme that it can be truly termed morbid. In one of Dr. Gull's patients, when the face was wiped with a towel, or the socks pulled on, the skin quickly became swollen and stiff with wheals.

These wheals are most readily produced in places where the skin is well supplied with muscular fibre-cells. No matter how susceptible it might have been before, when chloroform was dropped upon the skin no wheal could be brought out; when ice was applied to the part immediately after friction, a wheal did not arise; and by stretching the skin, a wheal could be obliterated, apparently by obliterating the contraction of its muscular tissue. Fictitious urticaria must, therefore, be the result of contraction of the muscular fibre-cells contained in the skin.

This communication is illustrated by a plate, showing two letters raised upon the arm. They have risen into bold relief, pale, and surrounded with a capillary blush.

XVIII. *Contributions to Dental Pathology.* By S. JAMES A. SALTER, M. B., F. L. S., F. G. S.

The first of the contributions is on the impaction of permanent teeth in the substance of the maxiliary bones. It not unfrequently happens that certain of the temporary teeth are found present and firm in the adult mouth, whilst the corresponding permanent teeth, those which should have succeeded and replaced them, have not made their appearance. In these cases, the presence of the temporary tooth is almost always associated, not with the total suppression and absence of the corresponding permanent one, but with its presence in an abnormal position. Seven cases are described in which this condition of things was

found to exist, each of them illustrated by a wood-cut. The histories of several cases are also detailed, in which the capsule of the impacted permanent tooth dilated into a serous sac. One of these cases is also illustrated by a wood-cut. This is a fact not to be lost sight of by the surgeon, that an expanding tumour in the antrum maxillare, or indeed in any part of the maxillary bones, may depend on a serous cyst involving a misplaced tooth.

The second contribution is on the subject of "Warty Teeth," of which an account was given in the analysis of the last volume of *Reports*. This contribution is likewise illustrated by two wood-cuts.

XIX. *Cases of Injured Arteries.* By Alfred Poland and Constantine Holman, M. D.

In the first of the cases reported in this communication, there was a traumatic aneurism of the ulnar artery in the palm of the hand, for the relief of which the artery was tied both above and below the sac, and still it pulsated as freely as before. After tying the radial artery so as to cut off the supply of blood from the deep palmer arch, all pulsation ceased immediately. In the second, the wrist was wounded by a cut from a knife, and although both ends of the radial artery, the only one that was thought to be injured, were tied, the hemorrhage still continued, and was only arrested by the ligature of the ulnar.

The third case is one of aneurism of the anterior tibial artery following an operation for the removal of the necrosed head of the fibula, probably from some injury then received by the coats of the vessel. An attempt to ligature the vessel at the seat of the aneurism was ineffectual, even after freely laying open the sac, on account of the great depth of the vessel and its retraction into the interosseous space; and it was necessary to tie the femoral, which was done at the upper third.

In addition to these three cases of injured arteries this communication contains the details of a case of dislocation of the hip-joint from strumous disease, where the head of the right femur was spontaneously cast off and found in an abscess below Poupart's ligament, from which it was removed.

XX. *On some points in the Diagnosis and Treatment of Heart Disease.* By G. H. Barlow, M. D.

This essay, though only five and a half pages in length, is one of very great interest, and is evidently the result of many carefully made observations and much reflection. It is written to point out that the greater number of valvular diseases of the heart, and a considerable proportion of diseases of the organs of circulation in general, have a tendency to range themselves into one or other of two large classes, according to which classification our treatment should be, in a great measure, regulated. The great difference between these diseases consists in the obstruction—for, after all, regurgitation is practically the same as obstruction—originating on one side or the other of that great barrier the mitral valve. The tendency of disease of the heart is to propagate itself backwards; that is to say, in the opposite direction to the current of the circulation. Now this tendency is, in the case of aortic disease, opposed, and often for a long time successfully opposed, by the perfect closure of the mitral valve. When, however, the mitral valve is affected, and its closure is imperfect, the lungs, right heart, and liver, become gorged in rapid succession.

In disease of the aortic valves, what we most dread, are death from syncope, and a gradual failure of the power of the left ventricle which becomes dilated. The plan of treatment indicated is tonic and often stimulating, the object being to maintain the tone of the system, and, as much as possible, to prevent this dilatation. In disease of the mitral valves, on the other hand, although the pulse is much more feeble, the same advantage is not derived from stimulants; we must rely here chiefly upon relieving the circulation through the portal system by free catharsis, establishing a free action of the kidneys, and relieving the pulmonary circulation by free expectoration.

XXI. *Cases Illustrative of the Etiology of Enlargement of the Heart.* By G. H. Barlow, M. D.

This communication contains the histories of the fatal cases of heart disease

that have occurred in Guy's Hospital for the past two years, in which there was no affection of the valves or adherent pericardium to account for its production. The causes of enlargement of the heart are very properly divided by Dr. Barlow into three classes, namely:—

1. Obstruction from changes in the orifices of the heart, or in the course of the circulation.
2. Obstruction arising from changes in the quantity or physical properties of the blood.
3. Deficiency of strength in the parietes of the heart itself.

The cases reported in this communication are seventeen in number, and they are mainly illustrative of the second and third of these classes. In a large proportion of them Bright's disease co-existed.

XXII. *On the Failures of Marsh's Process for the Detection of Arsenic.* By WILLIAM ODLING, M. B., F. R. S.

This communication is one of importance to those who may be called upon to make chemical investigations in cases of poisoning or others where the presence of arsenic may be suspected.

Attached to this volume is a page of errata, which is added for the purpose of correcting errors in the calculations of percentage in Mr. Poland's paper on Tetanus, in the third volume of the *Reports*. These were pointed out in the notice of that volume made in this journal. W. F. A.

ART. XXVIII.—*Pathological and Practical Observations on Diseases of the Alimentary Canal, Œsophagus, Stomach, Cœcum and Intestines.* By S. O. HABERSHON, M. D., London. Fellow of the Royal College of Physicians, Assistant Physician to Guy's Hospital, and Lecturer on Materia Medica and Therapeutics, etc. etc. 8vo. pp. 312. Philadelphia, 1859, Blanchard & Lea.

ALTHOUGH this volume is a small one, when compared with the magnitude and importance of the subjects of which it professes to treat, it will be found, nevertheless, to present a series of facts, derived from the clinical observations of the author, of a particularly instructive character—whether as confirmatory of deductions that had been already made in reference to the pathology and proper treatment of the diseases to which those facts refer, or as pointing to new ones that had escaped the notice of preceding labourers in the same field of observation.

The work of Dr. Habershon is not presented as a systematic treatise on the diseases of the alimentary canal, but rather as a concise, but at the same time clear exposition of such clinical observations as seem adapted to shed additional light upon those difficulties in respect to the nature, diagnosis, course, termination, and treatment of these diseases, which constantly present themselves to the physician in the daily practice of his profession; the deductions arrived at being enforced by the brief history of one hundred and sixty-three cases that had fallen under the notice of the author.

A considerable portion of the facts recorded in the chapters on diseases of the œsophagus, and on organic diseases of the stomach, had already appeared in the pages of the Guy's Reports for 1855 and 1856, and we are informed by Dr. H., in his preface to the present volume, that those, also, in connection with diseases of the intestines, were intended originally to be printed at the same time, had the limited space allowed to each contributor permitted this to be done.

The first, or introductory, chapter presents a hasty, but interesting sketch of the general pathology of the different portions of the digestive tube; of the mucous coat and its secretions; of the muscular coat, giving rise to an irregular, tumultuous, retarded, or spasmodic condition of the peristaltic action; and, lastly, of the serous investment; with a notice of the various derangements

of the circulation and innervation of these several parts. This pathological sketch is followed by a rapid review of the general principles that should guide us in the selection and administration of remedies for the control of the leading morbid conditions of the alimentary canal—whether adapted to check fermentative or other chemical action in the secretions from its mucous membrane and associated glands, to correct or improve these secretions, to remove offending or injurious materials or excretions, to restore the normal condition and movements, to alter the state of the circulation, and the condition of the bloodvessels and absorbents, and to modify the innervation.

A few instructive remarks are made upon the connection which often exists between different diseased conditions of the same or of different organs—either as mere coincidences, as the simultaneous impression of the same morbific influence upon several organs, or as different manifestations of the same disease in its progressive action. Reference is also made to the important modifying, predisposing, or antagonistic influence of one disease upon another; or the conservative influence exerted by one morbid condition against the occurrence of another.

From an attentive perusal of the entire chapter, many useful general hints will be derived, leading to practical lessons of the utmost importance.

The *diseases of the œsophagus* treated of in the *second* chapter, are spasmodic stricture, inflammation, organic obstruction, cancer, compression by aneurism or tumour, rupture of the coats and their solution by the gastric juice.

Many of the cases related in the present chapter as illustrative of these several lesions are particularly interesting, inasmuch as the several morbid conditions of the œsophagus are but very little understood. They are, not unfrequently, as will be seen by the observations recorded by Dr. H., very obscure in their pathology and most insidious in their origin. Some of the instances of ulceration of the œsophagus perforating the trachea or bronchi adduced by him, have generally, and, he believes, incorrectly, been described as examples of cancerous affection.

Under the head of *simple ulceration* of the œsophagus, it is remarked that—

"There are many instances of persons complaining of pain at the upper part of the sternum on swallowing, in whom no trace of pressure or aneurism can be found; and I have seen this symptom disappear under the use of tonics, sometimes with iodide of potassium. The idea of cancerous growth has been precluded; and it has been, therefore, a question whether some abrasion of the mucous membrane, or slight ulceration, such as we sometimes find in the pharynx, had not led to this complaint."

In regard to the treatment of simple ulceration of the œsophagus, the diagnosis of which, during life, is attended with the utmost difficulty, Dr. H. remarks that—

"It is exceedingly unsatisfactory; the spasmodic contraction of the ulcerated part prevents the passage of œsophageal tubes; no food can be swallowed, and the administration of nutrient enemata prolongs life only for a few days or weeks. It is painful to find, after death, that simple ulceration of the œsophagus, or a fistulous communication with the trachea, is the only existing disease, and that, if food could have been introduced beyond this point, life might have been prolonged. The operation of œsophagotomy is a very difficult one, and, in many of these cases, if performed, would be quite ineffective, because the disease is often situated at the root of the lung, or behind the first bone of the sternum; in either case, the operation could not be performed below the seat of stricture. It having been found that the peritoneum may be divided without fatal result, and without the terrible effect seen to follow from ruptured viscera, the propriety of forming a gastric fistula in some of these cases is worthy of very serious consideration. It appears certainly warrantable, as it would afford a chance of life to those who now have only a prospect of certain death. In the human subject, several cases of gastric fistulæ accidentally produced have been recorded, and the experimenters on animals purposely make such openings, under the influence of chloroform, without the production of severe peritonitis."

The diseases of the œsophagus are illustrated by the histories of twenty cases.

The *third* chapter is devoted to the *organic diseases of the stomach*—atrophy

of the gastric mucous membrane; post-mortem solution of the coats of the organ; inflammation [catarrhal, acute, and diphtheritic]; ulceration [superficial, follicular, aphthous, chronic, penetrating]; suppuration in the coats of the stomach; sloughing of the mucous membrane; fibroid degeneration of the pylorus; cancer of the stomach.

Dr. H. believes that, to some extent at least, the same forms of diseased action occur in the stomach as in the mouth—inflammatory congestion and acute inflammation, perverted epithelial growth and secretions, sluggish circulation, and fibroid and cancerous disease. Ulceration of the stomach he considers as probably a more common condition than is generally supposed, and that, in many instances, it will readily yield to a judicious treatment. Among the forms of gastric ulceration, he thinks that there are those which are superficial and evanescent, leaving after them scarcely any more trace in the mucous coat of the stomach, than aphthous inflammation of the mouth does in the mucous membrane of that cavity.

The cases related by Dr. H. show that there are several diagnostic phenomena by which the simple, follicular, and aphthous ulceration of the stomach may be distinguished from cancerous disease of the organ.

Fibroid degeneration of the pylorus, which has generally been considered as of a cancerous character, is, unquestionably, a form of disease having almost invariably a fatal termination, which the physician has it not in his power to prevent. Dr. H. has shown, however, that though we can neither remove the disease nor conduct it to a favourable result, we can do much to mitigate its symptoms, and to prolong the lives and comfort of those in whom it has become developed.

Referring to the investigations of Dr. Murchison on the subject of gastro-colic fistula, which were published while the work before us was in press, Dr. H. remarks that, in the two or three cases which have fallen under his observation, it appeared that cancerous growth in the stomach had led to adhesion between this organ and the omentum or colon, or both, new products being effused and the mucous membrane of the colon infiltrated—the perforation occurring as a consequence, rather of the disintegration of this secondary product, than from direct perforation from the part primarily affected. This explanation, he thinks, furnishes a reason why, in the cases recorded by him, no fecal vomiting took place.

In respect to cancerous disease of the stomach, the cases detailed by Dr. H. show that—1st, the symptoms may be exceedingly slight, and the presence and character of the disease overlooked. 2dly, that the indications are more marked where the gastric orifices are affected. 3dly, that, in most cases, death takes place from exhaustion or asthenia. 4thly, that fatal hemorrhage and perforation are more rare than in simple ulceration of the stomach. 5thly, that the absorption of degenerating cancer structure sometimes leads to symptoms resembling pyæmia. 6thly, that some of the distressing symptoms of cancerous disease of the stomach may be relieved, but excess of treatment has a tendency rather to hasten the fatal termination.

Thirty-six cases are given in illustration of the organic diseases of the stomach.

The *functional diseases of the stomach* are next considered (Chapter IV.). They are dyspepsia, pyrosis, hæmatemesis.

In respect to dyspepsia as simply a functional disease of the stomach, chemical research has already removed from it much that was formerly obscure, and cannot fail to throw still more light hereafter upon the nature of the pathological conditions which present themselves during its continuance. A more intimate acquaintance with the physiological connection of the sympathetic nerve, and the branches of the semi-lunar ganglia, will enable us, also, more correctly to estimate the very varied symptoms that occur in dyspepsia, many of which are produced by the morbid action of these portions of the nervous system.

Dyspepsia, according to Dr. H., is dependent upon either an abnormal condition of the digestive mucous membrane and its secretions; impeded muscular movements of the stomach; the state of the vascular supply; the condition of the nervous system; or the character of the food, and the changes which take place in it. Several of these causes may be combined in the same case. Some

of them give rise to merely slight and transient symptoms, while others of them are lesions of an irremediable character.

The entire account given by Dr. H. of the pathology and treatment of the different forms of dyspeptic ailments is well deserving of an attentive study. The views presented in respect to the causation and character of some of the most common and troublesome stomachic affections which fall under the notice of the physician, especially in our larger cities, are particularly sound, while the curative indications laid down by the author, and the means he points out for their fulfilment, although rapidly sketched, are worthy of especial consideration.

The observations on dyspepsia are illustrated by the histories of three cases.

Hæmatemesis, or vomiting of blood, is described under its several forms, accordingly as it results from ulceration of the stomach, from a congested or obstructed portal circulation, from vicarious menstruation, from cancerous disease, from a vitiated condition of the blood, as in purpura, and from aneurism.

Cases are adduced illustrative of hæmatemesis from cancer of the liver, from disturbance of the menstrual flux, etc., and one where the vomiting of blood succeeded to excessive indulgence in intoxicating drinks.

The *fifth* chapter is devoted to the *diseases of the duodenum.*

The facts which Dr. H. has deduced from clinical observations have convinced him that there are symptoms by which certain of the diseased conditions of this portion of the alimentary canal may be satisfactorily diagnosed; and yet, so intimately combined are the symptoms resulting from many of the morbid states of the duodenum with those indicative of simple disease of the contiguous viscera, that definiteness and certainty cannot always be attained. The chapter presents us with instances of disease of the duodenum which closely simulated disease of the pyloric extremity of the stomach.

Speaking of ulceration of the duodenum, Dr. H. remarks:—

"After burns the mucous membrane of the duodenum has been found greatly congested, and, in several cases recorded by Mr. Curling, in the *Medico-Chirurgical Transactions*, ulcerated. This has not been confirmed in the observations of Dr. Wilks, in the Guy's Report for 1856, many of which cases I witnessed; and although in some the first part of the duodenum was hyperæmic, in none did I observe ulceration. Such a case, however, is placed in the museum at Guy's, in which Dr. Gull has drawn attention to a small ulcer existing at this part. The child survived twenty-five days, but died comatose—a small cicatrizing ulcer was found in the first part of the duodenum.

"Mr. Curling describes diarrhœa, and the discharge of blood, as having arisen from this condition of the duodenum, and sometimes severe hæmatemesis and prostration. In some, death took place from peritonitis consequent on perforation. After such severe injury to the skin, it is not surprising to find great disturbance of the circulation of the internal organs and especially of the mucous membranes, which are known to sympathize so closely with the skin; in some of these cases, stimulants appear to have been administered freely, and these have probably conduced to the inflammation of the duodenum."

The histories of five cases are given in illustration of the diseases of the duodenum.

The observations recorded in the *sixth* chapter present examples of *gastro* or *muco-enteritis* and *acute enteritis.* In the first, the inflammation being confined to the mucous membrane of the intestine, with a tendency to spread along the course of the membrane, while in the second of these forms of disease, the inflammation, commencing in the intestinal mucous membrane, extends in depth rather than superficially, and, consequently, implicating the muscular and peritoneal coats with the connecting tissues.

The histories of three cases serve as illustrations.

In the very excellent account which Dr. H. has given of the inflammatory affections of the upper portion of the intestinal canal, the great importance of a correct diagnosis at the very onset of the disease is strongly urged upon the notice of physicians. Life may be readily sacrificed by loss of time as well as by improper treatment.

The erythematic or mucous form of enteritis will be conducted generally to a favourable termination by judicious diet, warmth to the abdomen, demulcents,

and other simple means. In the ordinary cases of enteritis, Dr. H. proscribes mercurials, while he urges in strong terms the value of salines—the bicarbonate and chlorate of potash and the carbonate of soda.

The subject of the *seventh* chapter is *strumous disease of the alimentary canal*, in its various forms. 1. As it occurs especially in infants—diarrhœa, with or without strumous disease of the mesenteric glands, often induced by, and in many cases leading to, uncontrollable purging. 2. Primary disease of the mesenteric glands. 3. Tubercle in the peritoneum, and strumous peritonitis, in its several forms—of *a*. Miliary tubercles covering the general surface of the peritoneum—visceral, parietal, and mesenteric. *b*. Strumous deposit on the peritoneum, and in the glands, associated with inflammation inducing a matting together of the intestines, sometimes to perforation from without, or to the formation of small fæcal abscesses. *c*. Peritoneal ascites with miliary tubercles. *d*. With tympanites. 4. Tubercle in the mucous membrane with enteritis, leading to softening, ulceration, and perforation, as is frequently observed in phthisis. 5. Tubercle in the appendix cæci.

The author calls attention to the fact, that no one of these strumous affections of the intestine or peritoneum are to be regarded as of a strictly local character, the whole of them being the result of a general perverted nutrition. He has shown that strumous disease in other organs is, in many instances, obscured by the more marked affection of the abdominal viscera. In the treatment of the strumous affections of the alimentary canal, Dr. H. strongly urges the avoidance of mercurials and drastric purgatives. It is very certain that the lives of many delicate children have been sacrificed by the administration in these diseases of vermifuge and other quack remedies.

The brief histories of six cases are given in illustration of the diseases embraced in Chapter VII.

A most interesting series of observations follow (Chapter VIII.), illustrative of diseases of the cæcum and its appendix. From these we learn that distension, local, circumscribed inflammation, and some other affections of the cæcum occur, in many cases, altogether independent of the more serious form of cæcal disease consequent upon perforation of the appendix.

From observations recorded in the chapter before us, it would appear that the symptoms of cancerous disease of the cæcum differ from those of simple cæcal inflammation and perforation, and that in many cases this difference in the symptoms will enable us to detect the true character of the complaint, which is a matter of some importance, inasmuch as, in cases of a non-malignant nature, much may be done, by a proper course of treatment, for the alleviation of cæcal disease, and to insure its favourable termination. The pain attendant upon the generality of these affections, points to the necessity of perfect rest, which should be continued without intermission for several days after the acute pain has subsided. To relieve the constipation which often attends the disease, purgatives are not adapted—they aggravate the malady without unloading the bowels. The application of leeches, with the use of opium internally, will, according to the experience of Dr. H., better succeed in securing the desired result. Mild mercurials, combined with opium, have a beneficial effect, we are assured, in hastening a subsidence of most of the morbid conditions of the cæcum.

The leading particulars connected with the pathology, diagnosis, course, and treatment of the several affections of this intestine and its appendicula are well illustrated by a series of appropriate cases.

The subject of the *ninth* chapter is *diarrhœa*. Although it is admitted that abnormal frequency in the evacuations from the bowels is merely symptomatic of certain diseased conditions of the intestinal canal, extremely various in character, Dr. H. has, nevertheless, considered it proper to present a description of the several forms under which morbidly frequent discharges from the bowels take place—with a general consideration of the characters which distinguish each of them—their prognosis, causes, and treatment.

Colitis and *dysentery* are discussed in the next chapter (Chap. X.). The more severe and malignant forms of dysentery, such as are met with in tropical climates, in particularly unhealthy localities, in military campaigns, and in

certain epidemics of the disease that have prevailed from time to time, are of comparatively rare occurrence in Great Britain, and the same remark will hold true also of most of the cities in our middle and northern States.

Dysentery Dr. H. defines to be an inflammatory affection of the intestinal mucous membrane, generally limited to the colon, and when severe, rapidly passing into ulceration or sloughing, unless, from its extent and violence, or the previous condition of the patient, it proves fatal at an early stage.

In regard to the division of dysentery made by Dr. Lyon, in his *Crimean Report*, into the exudative and the follicular, Dr. H. supposes that the first does not indicate a distinct form of disease, but rather, perhaps, an earlier period of one and the same form, or that preceding ulceration and sloughing. The cases of dysentery that have fallen under his own observation, may be divided practically, he remarks, into three classes. 1. Those in which the inflammation of the colon was the primary disease—where it was very extensive, and sometimes rapidly fatal. 2. Those associated with inflammation of other membranes or organs, arising at the same time, and produced apparently by a general cause, as with bronchitis, laryngitis, or pneumonia, in some instances, closely allied to pyemia. 3. Those cases in which inflammation of the colon has hastened the fatal termination of other more chronic diseases.

"In some of the cases, typhoid fever was simulated; in others, perforation of the colon had taken place; in one, there was pyemia and commencing suppuration in the liver; in several chronic cases, the secondary effects were shown in producing contraction of the intestine, perforation, and artificial anus, etc. As regards abscess in the liver, in one, the abscess had dried and contracted; in another, fresh diseased action was set up around it, and abscess in the brain was the result."

In its most severe forms, dysentery will be found to prove almost invariably fatal; the utmost that can be hoped from the best devised treatment is to soothe in some degree the more painful symptoms, and thus relieve the patient of a part of his sufferings. In the great majority of the cases which usually present themselves, however, by demulcents given by the mouth and in the form of enemata; astringents similarly administered, with opium, ipecacuanha, etc., appropriate clothing and diet, avoidance of cold and dampness, we shall be able, with considerable certainty, to stay the progress of the disease, and to shorten its duration.

Dr. H. has never met with a case of dysentery in which venesection could have been practised with probable success. He considers that local bleeding, however, is highly serviceable, either from the anterior surface of the abdomen or from the anus. Leeches to the latter part, it is certain, will often relieve the violence of the pain and of the tenesmus.

Fifteen cases are detailed in illustration of the teachings of the chapter.

Dr. H. has omitted all notice of Asiatic cholera. This he has been induced to do, because it has not been clearly shown that the disease is really one affecting alone or principally the abdomen, while at the same time he considers that the report of Drs. Baly and Gull presents, with sufficient accuracy and clearness, all the facts known in reference to the disease.

In the chapter on *typhoid fever* (Chap. XI.), only the abdominal lesions characteristic of the disease are described; neither the pathology of the fever itself nor its treatment is discussed. Dr. H. merely warns against the exhibition of opiates for the purpose of arresting diarrhœa, from their liability to produce cerebral congestion and excessive engorgement of the lungs.

The chapter (XII.) on *colic* is devoted to a brief notice of the simple and more manageable forms of the complaint—those, namely, which result from flatus—occurring in nervous subjects, and attended with spasm of the bowels—or such as are caused by some injurious property in the food, by morbid secretions, or retained excretions, and by poisoning with lead. The more severe and unmanageable forms of painful intestinal disease, such as ileus, internal strangulation, intussusception, and cancerous affection of the colon are treated of separately in the *fourteenth* chapter.

In reference to these latter affections, Dr. H. remarks, that, while they all bear a strong general resemblance in the production often of fatal constipation,

they may, in many instances, be distinguished from each other. Simple colic may be accompanied by a discharge of bloody mucus, or by actual diarrhœa. The latter symptom is often connected also with cancerous disease of the sigmoid flexure of the colon. In all of the diseases embraced in the fourteenth chapter, Dr. H. enters his protest against the use of every kind of purgative, and as earnestly recommends opiates in large doses. The cases detailed by him show, almost uniformly, that when purgatives, even the mildest, were given, the vomiting, pain, and distress were all increased, whilst these as well as the other symptoms present were, on the contrary, relieved by the free use of opium.

The *thirteenth* chapter treats of constipation. Dr. H. enumerates as causes of this condition of the bowels, peculiarity of habit or idiosyncrasy; paralysis, diminished contractility, or excessive fatness, etc., of the abdominal walls; alteration of the secretions poured into the large intestine; abnormal condition of the coats of the intestine itself; impacted condition of the large intestine, from a combination of feces with indigested portions of certain kinds of food habitually taken; and, finally, mechanical obstruction from tumours affecting the coats of the intestines, or more generally from the pressure of the gravid uterus, ovarian growths, etc.

Among the results of protracted constipation pointed out by Dr. H., are enormous distension of the intestine from continued accumulation of feces, and the abnormal development of the lateral pouches of the colon. In this condition the contents of the intestine are to some extent removed from out the central current, and may remain impacted in them while the stools are passed with some degree of regularity. The enlarged pouches filled with impacted feces may frequently be felt like tumours through the abdominal walls.

"These pouches of the colon sometimes become of considerable size. Generally the circular fibres of the canal surround them, but not very unfrequently the circular fibres yield, and the mucous layer projects, covered only by the peritoneum, forming a more elongated sac, filled with mucus, or more frequently with feces. The orifices of these small sacs are bounded by the hypertrophied circular and longitudinal fibres, and their contents remain almost shut off from the intestinal canal."

Dr. H. has most frequently observed the distended pouches in connection with the sigmoid flexure; but, as he remarks, they probably occur at any part where the longitudinal fibres form a triple band rather than a uniform layer. In one case, they were situated about every half inch, forming a double row on each side of the colon. No muscular fibres could be detected in several of them, beyond the immediate vicinity of the mouth of the sac, but merely mucous membrane, submucous cellular tissue, fat, and peritoneum. These pouches do not appear to produce any symptom, or lead to dangerous results.

A brief chapter (Chap. XV.) is devoted to the subject of *worms*. It is the least interesting and instructive one in the whole volume.

The subject of the *concluding chapter* is *suppuration of the abdominal walls* and *perforation of the intestine from without*. The series of observations adduced in illustration of these by no means unfrequent occurrences are especially worthy of a close study. Many of the cases present great obscurity in their diagnosis, and vary much in the course they pursue. To make out the exact nature of the morbid conditions present in each, the utmost care is to be observed, and the import of every symptom present weighed with the utmost care.

Their treatment must be based upon indications derived from the character of the prominent symptoms of each case, as they become developed. The entire subject of pathology and management is well illustrated by the histories of the eleven cases which are given by the author.

To those who desire to consult a series of admirable observations well adapted to throw important additional light upon the general symptoms, pathology, and treatment of the leading diseases of the different portions of the alimentary canal, the work of Dr. Habershon is very confidently recommended. It is replete with instruction invaluable to the young practitioner, and from which even the most experienced of the elders of the profession may gain some facts that have escaped them in the prosecution of their own clinical investigations.

D. F. C.

ART. XXIX.—*The Mortality after Operations of Amputation of the Extremities, and the Causes of that Mortality.* By ARTHUR ERNEST SANSOM, Associate of King's College. The Prize Essay of the Medical Society of King's College, London, for the year 1858. London: John Churchill, 1859. 8vo. pp. 23.

THE very great importance of the subject, and the desire of facilitating the labours of more competent observers than himself by an accumulation of data, are assigned by the author as the reasons for the publication of this interesting little pamphlet. He conceives, moreover, that in the labours of the learned on this subject, "pet theories may creep in, mingling even with statistics, though a would-be watcher be over them," and that "there may be too something beneficial in truth being sought by one so young in the profession, untrammelled by prejudice."

The great mortality attendant upon amputations was first made known to the profession in this country by the article published in this journal in August, 1838 (vol. xxii. old series), entitled "*Statistical Account of the Cases of Amputation performed at the Pennsylvania Hospital*, from January 1st, 1831, to January 1st, 1858, by George W. Norris, M. D., one of the surgeons." We might almost say was first made known to the profession at large, for with the exception of an essay published by Mr. B. Phillips, of London, in the *London Medical Gazette* for June 9th, 1838, we believe that no information of any great importance had ever before been presented on this important subject to the medical public. Since then two additional articles have been published by Dr. Norris in this Journal, one in vol. xxvi. (1840), the other in vol. xxviii. N. S. (1854). The statistics of the amputations performed in the Massachusetts General Hospital have also been published in this Journal, in articles contributed by Dr. Hayward, in vol. xxvi. (1840), and vol. xxi. N. S. (1851). Those of the New York City Hospital, drawn up by Dr. Buel, are to be found in vol. xvi. N. S. (1848). Others of the same hospital were furnished by Dr. Lente in the *Transactions of the American Medical Association*, vol. iv. (Philadelphia, 1851), and an abstract of them was given in the number of this Journal for April, 1852. The statistics, therefore, of the amputations performed in the chief hospitals of our great northern cities, and the great mortality attending such operations, are well known to the medical profession in this country.

Among the most, if not indeed the most, important papers ever written on this subject of the mortality of amputations, are those of Dr. Samuel Fenwick, of New Castle upon Tyne, contained in the *Monthly Journal of Medical Science*, in the numbers for 1847 and 1848. We esteem them so highly not only because the statistical tables given therein are very extensive, but also on account of the useful practical facts deduced from them by the author. His tables show the general mortality of amputations of the limbs, the influence of age, of sex, of the cause requiring the operation, and of the season of the year, the time after an operation when the danger to life is greatest, the number of deaths caused by different diseases after amputation, the time at which life is generally destroyed by these diseases, how the success of an amputation varies according to the part of the limb at which it is performed, the influence upon the mortality of the nature of the accident or the disease for which it was performed, the influence of the duration of the disease, the success following primary and secondary operations, the influence of the circular and the flap methods, and what are the causes of death after each. These, though perhaps the most, are by no means all, the interesting and instructive points elucidated by the tables contained in these papers of Dr. Fenwick, which might well be taken as models by those who desire, by statistical researches, to resolve or aid in the solution of surgical problems.

In some remarks we took occasion to make, when reviewing recently the small volume of Mr. James on the operation for strangulated hernia, we called attention to the great mortality, as was shown by statistics, attending this operation,

which was generally believed, but a few years since, to be one of comparative harmlessness. In like manner surgeons were far from considering the operation of amputation to be so fatal as the statistics of Norris and others above mentioned have shown it unquestionably to be. Bell in his celebrated system of surgery declares that not one death will happen in twenty cases, even including the general run of hospital practice; and that in private practice, when due attention can be more certainly bestowed upon the various circumstances of the operation, the proportion of deaths will be much less. The tables of Dr. Fenwick show that out of 4,937 cases, 1,565 died, or that the operation was fatal in 1 case in every 3.15. In fact, the horrible mortality that attends all capital operations was never dreamed of before statistics were applied to surgical investigations, and we can scarcely estimate too highly their value. We would wish here to make a remark that seems to us to be called for. Like all good things, statistics have been abused, and the knowledge derived from them in regard to the high mortality attending capital operations, has been improperly applied. We cannot, perhaps, express our meaning better than by quoting from the papers of Dr. Fenwick, above referred to. He says: "An attempt has been made to sanction other operations, by comparing their results with those of amputations; and, were the reputation of the surgeon the only matter for consideration, no better means could be employed for that purpose; but as it is the value to the patient which is to be estimated, the matter must be viewed in a very different light. Thus, although one out of every four cases of removal either of an ovary or limb may die, the value of the operation will be very different to the persons suffering from the complaints requiring the performance of these operations. We are here to weigh against the dangers of extirpation, a considerable chance of two or three years' existence, and a comparative freedom from pain during that time; whilst, in the other, a speedy death or great suffering is all that can be expected, unless by amputation the affected part be removed."

The statistical tables prepared by Mr. Sansom are compiled partly from Mr. Guthrie's commentaries, partly from the records of the London Medical Society of Observation, and partly from the records of individual operations published periodically in the *Medical Times and Gazette*. The table of the operations performed in London from 1837 to 1857 shows that in London practice the mortality after amputation, in the last twenty years, has been 29.86 per centum. The table as given by Mr. Sansom is as follows:—

PERIOD.	No. of operations.	Deaths.	Per cent.
1837–1842	184	69	32.95
1840	87	32	36.07
1854	180	56	31.11
1855	136	35	25.75
1856	155	43	27.74
First six months of 1857 .	84	19	22.61
January, 1854 to June, 1857 .	555	153	27.05

There are some mistakes in the last column of this table which should be corrected; 32.95 should be 37.50, 36.07 should be 36.78, and 27.05 should be 27.56. In the tables of Dr. Fenwick are collected 3,486 cases, occurring in civil practice in Great Britain, the United States, France, and Germany; of these 1,117 died, or 1 in 3.12, giving a mortality of 32 per centum. This we think is nearer the true percentage than that of 23 per cent., given by Dr. Gross, in his late work, and the results, therefore, of London practice, must be considered as quite favourable.

A very interesting question is the comparative risk attending amputations and excisions. What Mr. Sansom says on the subject is, that in 1855, 1856, and the first half of 1857, there have been performed 69 excisions of the principal joints; the deaths have been 13; thus making a mortality of 18.84 per centum. Consequently, according to the statistics given by Mr. Sansom, the

risks attending the operation of amputation are as much as one-half higher than those attending that of excision; these are to each other as 27 : 18 or as 3 : 2. These statistics are very insufficient, and give, we believe, an erroneous impression. Out of 160 cases of excision of the knee, collected from all British sources, both metropolitan and provincial, by Mr. Price, 32 died, or 1 in 5.[1] Mr. Bryant, in a paper, an abstract of which was given in the July number of this journal, states that at Guy's Hospital, of 167 cases of pathological amputation of the thigh, 18 per cent. were fatal, or 1 in 5½. Taking into consideration the greater fatality attending operations in cities than in the country, and above all in a city hospital, we think ourselves right in concluding that, so far as the knee-joint is concerned, excision of the joint is, at least, equally fatal with amputation of the limb. We must not, moreover, lose sight of the fact that in 17 of the cases that recovered from excision, amputation became afterwards necessary.

Several pages of Mr. Sansom's pamphlet are occupied with discussing the influence of anæsthetic agents upon the mortality after amputations. This question, upon which at the present time accurate information is so much desired, it is evidently the wish of Mr. Sansom to decide in favour of the employment of these agents. He adds but very little indeed that is new to the statistics given by other British surgeons on this subject, and these he regards in the light most gratifying to himself. In this way he is led to the conclusion "that in cases of disease where amputation is necessitated, confidence in chloroform should remain unshaken; but that in cases of severe injuries it behooves at least to ponder well before its use." The following table is given as showing the mortality per cent. after amputations for diseases of bones and joints, as deduced from the tables of the London Medical Society of Observation.

	All members.	Thigh.	Leg.	Upper ext.
PRE-CHLOROFORM PERIOD—				
1837–1842	33	29	44	23
1840	34	43	25	40
POST-CHLOROFORM PERIOD—				
January 1856 to July, 1857	19	20	17	14
1854 and 1855 . . .	20	20	23	8

The post-chloroform period in Mr. Sansom's table does not comprise a sufficient length of time to justify any positive deduction to be made from his observations. To show the necessity of extending the observations through a number of years, Dr. Norris, in the statistics above mentioned, calls attention to the fact that from January 1st, 1830, to January 1st, 1832, the mortality was 1 in 11; from January 1st, 1832, to January 1st, 1834, it was 7 in 14; in the next two years 8 in 15; in the next two 5 in 15; and in the next two, or from January 1st, 1838, to January 1st, 1840, it was 1 in 24. Now these were similar cases, and they were subjected to similar influences and treatment. From a table given in the review of the *Transactions of the American Medical Association* in this journal for April, 1852, it appears that, according to the statistics then available, the mortality with anæsthetics in the American hospitals was 1 in 2⅓, or 43 per cent., in cases where amputation was performed for injuries, and 1 in 4⅝, or about 20 per cent., when performed for diseases. Without anæsthetics the mortality was 1 in 3$\frac{37}{88}$, or 29 per cent., for injuries, and 1 in 6¾, or 15 per cent., for diseases. More facts on this important point are very much to be desired.

In regard to the average periods of recovery and death in amputations of the upper and lower extremities, Mr. Sansom's table shows that in the case of amputation of the upper recovery is more tardy, and the advent of death more rapid. This, to use his own expression, is *strange*. The number of the cases from which this calculation has been made is not stated. As a general rule, according to the tables of Dr. Fenwick, amputations of the upper extremity

[1] We cite from the British and Foreign Medico-Chirurgical Review, for October, 1859, page 440.

heal more quickly than those of the lower, and, in either, the time required for cure depends considerably upon the thickness of the parts at which the operation is performed.

That portion of Mr. Sansom's pamphlet treating of the causes of death after amputation is based upon the results given in a table calculated from the cases recorded in the London hospitals from January, 1856, to June, 1857. His conclusion is that the two most common causes of death after amputation are shock and exhaustion, and pyæmia, "that the former is now-a-days the most frequent, being twice as frequent as the latter." Here again it must be said that the data furnished are altogether insufficient for arriving at any correct general conclusion, such as is implied by the expression *now-a-days*. Statistical tables of much greater extent go to show that phlebitis and purulent absorption occur in the majority of the cases of amputation terminating fatally. The two causes of death in amputations mentioned by Celsus are *profusio sanguinis* and *defectio animæ;* modern surgery, by the use of the ligature, contends successfully with the former danger, but nervous shock is as much to be dreaded as ever, and we despair before pyæmia, a danger in ancient times entirely unknown.

Speaking of Celsus leads us to endeavour to rectify what we believe to be an erroneous statement in regard to him, made by Mr. Sansom in a historical resumé of the most special circumstances affecting the increase or decrease of the rate of mortality after amputation. He says that "it was not until Celsus that there was any improvement; he extended slightly the list of circumstances under which the operation should be undertaken, and he distinctly specifies in his work a method of arresting hemorrhage by ligatures on the bleeding artery." Now the fact is that Celsus does not speak of amputation anywhere but in the 39th section of the VIIth book, and the only cases in which he advises it are in certain cases of gangrene of the extremities. As to ligaturing the arteries in amputations, he certainly never thought of such a proceeding. Bleeding should be arrested, according to him, by sponges with vinegar and water, and applications of a similar character. He adds, moreover, *cetera postea sic facienda, ut in vulneribus, in quibus pus moveri debit, præceptum est.* Now in the place thus referred to nothing is said of ligaturing bleeding vessels. The only place where Celsus speaks of tying a bloodvessel is where he speaks of *curable wounds*, and then he states that in obstinate bleeding from an incised wound two ligatures may be tied on the *vein*, which should afterwards be divided between them. (Lib. V. Sect. xxvi. § 21.[1]) This point is, indeed, not one of much importance, but it possesses considerable interest in a historical point of view.

In what we have taken occasion to say of this little publication, we hope that we may not be misunderstood when objecting to some of the tables therein contained as being insufficient. Though we must hold them to be not sufficiently extensive to warrant general conclusions to be made therefrom, yet we must heartily thank the author for their preparation, and for having presented them to the medical public. Such publications as his are most welcome to those engaged in investigating one of the most important points in surgery, the dangers of amputations, and the best means of contending with them. W. F. A.

ART. XXX.—*Urinary Deposits; their Diagnosis, Pathology, and Therapeutical Indications.* By GOLDING BIRD, M. D., F. R. S. Edited by EDMUND LLOYD BIRKETT, M. D., etc. etc. A new American from the fifth London edition. With eighty illustrations on wood. 8vo. pp. 382. Philadelphia: Blanchard & Lea.

VERY few works are better known to the medical profession than that of which the fifth edition is now before us; very few have established for their

[1] The edition we have referred to is that of Leonard Targa, Leyden, 1785, admitted to be the best ever published.

authors a more solid claim to a place among the *emeriti*. Dr. Bird's memory will ever be honoured as that of an earnest, intelligent, and successful labourer in a most important field.

The duty of preparing this posthumous edition of a work so valuable to the student and practitioner of medicine has been well performed by Dr. Birkett. We have, of course, here to call attention only to the modifications he has made, and of these we can only allude to the most important. They are partly incorporated in the text, and partly arranged in the form of foot-notes.

In the first three chapters, on the chemistry and physiology of the urine, we find some new matter of moment. The description of two acids discovered in the urine by Dr. Marcet, published by him in 1853, is quoted in full, with its accompanying wood-cut. The processes recommended by Liebig, Lehmann, and Davy, for obtaining and estimating the amount of urea, are detailed; as well as that of Lehmann for determining the quantity of chlorine, and that of Bensch for determining the amount of uric acid.

The tables in § 26, for the chemical and microscopical examination of urinary deposits, have been rendered much simpler and more convenient, and to a slight extent enlarged.

On page 61, a paragraph is inserted on the subject of the new views entertained by chemists with regard to the organic radicals. A more extended exposition of this important change in theory would have been by no means out of place here, while it would have proved very valuable to the less proficient reader.

Dr. Bird, in the previous editions of this work, expressed the opinion that the principal salt of uric acid, obtained from healthy urine, was that formed with ammonia. His present editor, while acknowledging the ingenuity of the theory sustaining this idea, denies its correctness; preferring the view held by Lehmann, that the salts of uric acid with soda, lime, and potash, are the important ones, while the urate of ammonia is present only in very small quantity. Five cases, observed by Drs. Hassall and Letheby, are adduced in support of this preference, which is in accordance with the views expressed by Simon in his "Chemistry of Man," and with those set forth in the recently published work of Thudichum on "The Pathology of the Urine."

In a note appended to Chapter IX., on oxaluria, the views of Lehmann, Scherer and others as to the non-existence of an oxalic acid diathesis are stated, and the arguments in favour of its occurrence are concisely summed up. Such a diathesis exists, in the opinion of Dr. Bird and his editor, and we believe the profession in this country are disposed to agree with them upon this point.

In the chapter on the phosphates, the statements of Robin and Verdeil in regard to the neutral and acid phosphates of soda are quoted, with cuts copied from their representations of the crystalline forms of those salts. Another note to this chapter awards Dr. Hassall the credit of having first described the crystals of phosphate of magnesia.

To the next chapter, on deposits of abnormal blue or black colouring matters, we find added Dr. Hassall's views in reference to the relation between indigo and hæmatin, and a notice of Dr. Hughes' cases of black urine, published in *Guy's Hospital Reports*.

We now come to the portion of the work devoted to non-crystalline organic products. And here we find, in the first place, a note on the various forms of casts, with several wood-cuts. So far as it goes, this note is very good; but it seems to us that more space might have been allotted to so very important a subject, and especially to the collocation of the clinical symptoms with the forms of casts usually associated with them.

Several notes are introduced in the section on saccharine urine. Horsley's chromate of potash test, Luton's test, and the fermentation test, are detailed. Dr. Beale's remarks on the causes of failure in the use of Trommer's test are quoted, and will be found useful to many. But the most important portion of this additional matter is that relating to the physiological and pathological origin of sugar.

And here we must express our regret that the editor has failed to award Bernard, perhaps the greatest physiologist of our day, that credit to which he is

entitled for his labours in the investigation of this subject. Dr. Pavy is made, as it were, to take the matter out of Bernard's hands.

Nor do we find ourselves much enlightened by the remarks which the editor offers as setting forth the existing aspect of the subject. We have space here to criticize, but not to attempt the much more difficult task of doing better; and we must allow that we should shrink from such an undertaking until further light is obtained.

It must be a matter of surprise that no allusion has been made in any edition of Dr. Bird's work to a paper on kiestein, published by Dr. E. K. Kane, in this journal, in July, 1842. Being based on clinical observation, and written in a philosophical spirit, that paper was surely worthy of reference in our author's bibliographical table, as a companion to the article on the same subject which he put forth in 1840, in the *Guy's Hospital Reports*.

The form in which this volume now appears is a very attractive one. The page has been considerably enlarged, and the type is clearer and more agreeable than that employed in previous editions. A very few typographical errors call for correction, such as "Donne" for "Donné," "Dichholz" for "Eichholz," "materies *morti*."

In conclusion, we confidently predict that the present issue of Dr. Bird's work will be received by the medical public in America with even greater favour than its predecessors. J. H. P.

ART. XXXI.—*Gustaf von Düben's Treatise on Microscopical Diagnosis*, with 71 engravings. Translated, with additions, by Prof. LOUIS BAUER, M. D., M. R. C. S. Eng., etc. 8vo. pp. 82. New York: John Wiley, 56 Walker Street. 1859.

WHILE we entertain a very high estimate of the value of the microscope as a means of research, we look upon its use in diagnosis as mainly incidental and corroborative. Without this instrument, anatomy, physiology and pathology could never have made such advances as they have within the last twenty-five years, nor, deprived of its aid, could they progress as we confidently expect them to do in the future.

A microscopic examination may disclose the nature of a urinary deposit, or confirm a doubtful diagnosis in regard to a tumour; but it can hardly be requisite for distinguishing skin-diseases, leucocythemia, or other disorders of the blood, or affections of the viscera generally. Hence, of the seventy-six pages of reading matter comprised in the little volume before us, the first fifty-five are devoted to matters belonging rather to general pathology than to diagnosis. The book presents a compilation of facts, doubtless useful in their place, although they merit fuller discussion than its limits allow them, but not answering at all to the title selected. Nor, we must observe, is sufficient stress laid upon the diseases to be diagnosed, in that portion of the volume which deals with discharges from the uro-genital organs. It is, after all, only through the chain of phenomena leading up to the primary morbid condition, that any symptom is of value in diagnosis; to detect oxalate of lime in the urine is useless, without a knowledge of the antecedents of the deposit.

A rigid critic might take exception to numerous idiomatic expressions retained in the translation; nor are the illustrations as well-executed as we think they should have been. J. H. P.

Art. XXXII.—*Elements of Medical Jurisprudence.* By Theodore Romeyn Beck, M. D., LL. D., &c. &c.; and John B. Beck, M. D., &c. &c. Eleventh edition. With Notes by an Association of the Friends of Drs. Beck. The whole revised by C. R. Gilman, M. D., Professor of Medical Jurisprudence in the College of Physicians and Surgeons of New York. In two volumes. Philadelphia: J. B. Lippincott & Co., 1860. 8vo. pp. 884 and 1003.

This new edition of the great work of Drs. T. R. and J. B. Beck is the result, as the title-page informs us, of the good offices of a number of their surviving friends. "After the death of T. Romeyn Beck," says the preface of Dr. Gilman, "it was ascertained that he had, with characteristic industry, collected a large amount of matter for a new edition of his treatise on Medical Jurisprudence. These materials were by his family placed in my hands, with a request that I would prepare a new edition for the press. Conscious of my own inability to do justice to such a trust, I sought aid from the friends of Dr. Beck. The required assistance was cheerfully rendered; and I was soon enabled to place most of the more important chapters in competent as well as friendly hands. In this way, I hope that the public are assured of a good edition of the book; while the friends of the author have a very welcome opportunity to pay a sincere and well-deserved compliment to the memory of a wise and good man."

The editor further expresses the just hope that the list of names subjoined of the gentlemen who united with him in this labour of love "will serve as a guaranty that something has been done in this edition, if not to elevate the character, at least to add to the usefulness of a work which, at home and abroad, has been recognized as an honour to the medical literature of our country." His own task, "besides general supervision, has been to incorporate into the body of the work the materials prepared by Dr. Beck, and to make the changes of which he had indicated the propriety. Some things I have added, some things modified; but I trust that all has been done with due respect to the memory of the dead."

The collaborators are Dr. D. Tilden Brown, Resident Physician of the Bloomingdale Lunatic Asylum, who contributes important alterations and additions to the chapter (XIII.) on mental alienation; Dr. R. H. Coolidge, Assistant Surgeon of the United States Army, who has revised and added to the chapter (I.) on feigned diseases, and to that (II.) on disqualifying diseases; Dr. Austin Flint, to whom were intrusted the three chapters (III., IV., and V.) on impotence and sterility, doubtful sex, and rape, respectively; Dr. B. W. McCready, Physician of Bellevue Hospital, who took charge of the chapters (XVI. and XVII.) on poisons and on irritant poisons; Dr. Samuel St. John, Prof. of Chemistry in the College of Physicians and Surgeons, New York, to whom were referred the remaining chapters (XVIII., XIX., XX., XXI.) on irritant poisons, on narcotic poisons, and on narcotic acrid poisons; Dr. John Watson, who attended to the chapter (XV.) on wounds of the living body; Professor J. P. White, of Buffalo, who edited the chapter (VI.) on pregnancy; Professor John C. Dalton, Jr., who contributed a valuable paper on the Corpus Luteum; and, lastly, Messrs. George Shea and Murray Hoffman, of the New York bar, to whom acknowledgments are rendered "for valuable suggestions on the purely legal portion of the subject."

We need hardly say that these gentlemen appear to have acquitted themselves well in their respective tasks; and it would be difficult, if not invidious, to dwell upon the amount and character of the additions which each one has presented in his particular province. The general effect is such as to bring once more into active usefulness one of our best and most interesting works, long the most comprehensive and complete upon its subject in the English language; and one which, as a work of reference, we hope to see kept up in its character, as well as within the reach of medical jurists, for many years to come.

In glancing over the pages, we recognize many interesting notes and other valuable improvements; but, although an amount of condensation has been effected in the new matter, which might, in many places, prove no less advan-

tageous to the old, we would nevertheless have been glad to find more notice taken of several important cases which would have arrested the attention of the venerated author, and would doubtless have been noted with the fulness and acumen which have given his work its well-known stamp of reliable authority and encyclopedic character. Such cases, for instance, as that of Bocarmé, and those of Palmer, of Dove, and others, would hardly have been passed by Dr. Beck with the bare mention which seems to be the only notice afforded to them in the present edition; although, since its preparation for the press began, the journals have rarely been without some allusion to these or kindred cases in their pages.

On many of the topics, however, it is proper to say, that there is a decided advance on former editions; especially in the chapters on mental alienation, on wounds, on medical evidence, on feigned and disqualifying diseases, and on the questions in which the sexual functions and relations are concerned. The difficulty of introducing much new matter into the already crowded chapters on poisons, without great labour in condensation and inconvenient increase of length, has probably discouraged the discussion of some of the new cases under this head; but it has not interfered with the introduction of the generally received tests and of all such additional data as may be considered essential to the due progress of the work in accordance with the present state of toxicology.

The notes of Dr. Gilman are interspersed in considerable number, but with marked brevity, throughout the two volumes; and although not always in a very sober style, are clear and to the purpose; while they indicate great fidelity to his trust, as well as judgment, and familiarity with the whole subject.

We regret to find many typographical errors in a work where accuracy is a characteristic of paramount necessity. In all other respects, however, the getting up of the volumes is much superior to that of their predecessors, the printing being good, and the paper so much thinner, that notwithstanding some seventy-four additional pages, the actual bulk has been materially diminished. We know of no single work on a subject relating to medical science, that is so full of varied and absorbing interest to every class of readers; while as a work of reference to the professional inquirer it may still claim its long established position as among the best in the English language. E. H.

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES

IN THE

MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *Experiments on some of the Various Circumstances Influencing Cutaneous Absorption.* By Augustus Waller, M. D., Prof. Phys. in Queen's Coll., Birmingham.—In some former experiments[1] I endeavoured to elucidate the phenomena of cutaneous absorption on the lower animals (batracia), by immersing the hinder extremities in various solutions, and afterwards watching the period at which the absorbed substances reached the tongue, where their presence was detected by means of some reagent applied to its surface; as, for instance, a salt of iron, when the legs were immersed in a solution of yellow ferrocyanide of potassium; Prussian blue was then formed as soon as the ferrocyanide was brought to the tongue.

Furthermore, I was able to detect, by the aid of the microscope, the "lieux d'élection," or preference spots, where the cyanide escaped from the vessels.

On the present occasion I shall endeavour to elucidate cutaneous absorption on the higher animals, and, if possible, to give a more definite view of this function, by determining, by accurate measurement, the degree of rapidity, the peculiarities, &c., which it may offer in various conditions.

A very simple mode of demonstrating the existence of cutaneous absorption is by immersing the leg of a young guinea pig, not more than half-grown, into a mixture of equal parts of chloroform and tincture of aconite. After fifteen minutes' immersion, the part will be found insensible at the surface and extremities, and, after a short time, symptoms of poisoning by aconite will supervene, viz., nausea, efforts at vomiting, sometimes vomiting of bile, coldness of the surface and extremities, circulation very weak, laborious respiration, slight convulsive symptoms, and death.

The influence of age, or of thickening of the cuticle, is easily seen in the same way; for, if instead of a young animal we take an adult one, we obtain no poisoning, but merely local insensibility and slight disturbance of respiration, &c.

Another not less instructive experiment consists in replacing the mixture of chloroform and tincture of aconite by simple tincture of aconite. In this case, the limb may be indefinitely immersed without our obtaining either local insensibility or death, or indeed any symptom whatever of the presence of aconite in the system.

A fourth experiment, which consists in dividing the sciatic nerve, shows the influence of innervation on the function of absorption; for, if performed on an adult animal, and consequently one incapable of absorbing aconite in quantity

[1] Waller, "Absorption of various Substances through the Skin of the Frog."—*Frorieps Tagesberichte*, 1851.

sufficient to cause death, the powers of absorption will be generally found so much augmented that the animal will be poisoned by immersion of the limb in simple tincture of aconite.

In this experiment I attribute the acceleration in the cutaneous absorption to the paralysis of the bloodvessels, as in my experiments on the sympathetic nerve, where I showed that in bloodvessels the passage of the blood is completely regulated by nerves springing from the spinal cord. When the vascular nerves are paralyzed, the artery becomes greatly distended, and the blood flows faster within it. The foot after the section of the sciatic is, on this account, more hot and red; and for the same reasons it is easy to account for the more rapid absorption of medicinal agents.

A fifth experiment consists in placing a ligature on the limb, in order to impede the powers of absorption of the animal. Although the ligature does produce this result, I was rather surprised to find how much less efficient it was than is generally represented; for, whenever the least symptoms of a toxic influence made their appearance, a ligature placed over the limb rarely succeeded in saving the animal.

In order to obtain results more susceptible of measurement, I proceeded to substitute atropia for aconite, and to make use of the albino rat in lieu of the guinea pig. By this means, I possessed an agent whose intervention was immediately detected by its action on the iris. My choice of the albino rat was for the like reason, *i. e.* the facility which it offered for exact and easy measurement, in which respect this animal is far preferable to any other with which I am acquainted, unless we except the white mouse, which, however, is so liable to die from slight causes, that it is little adapted for most physiological experiments.

The *modus operandi* which I generally adopt is to immerse the limb into a small two-drachm bottle containing sufficient of the mixture to cover the foot and part of the leg. The strength of the solution of atropia being generally that from half a grain to one drachm of some menstruum, such as chloroform, alcohol, &c., I generally prefer simply to hold the animal during the experiment to any other mode of restraint. By these means I am able to guard against several causes of error, such as the direct contact of the solution with the eye or mouth, and, at the same time, avoid any unnecessary discomfort to the animal.

Chloroform and Atropia.—A solution of atropia in chloroform will generally be found to cause dilatation of the pupil after the foot has been immersed from two to five minutes. The dilatation, having once commenced, is usually very rapid, and the pupil very soon attains double or treble its normal diameter, which is about one-fourth to one-half a millimetre during daytime. It is easy to recognize that this dilatation is not in very simple ratio to the time occupied in its expansion, the expansion of the pupil being more nearly in proportion to the square of the time occupied than in a simple arithmetical ratio. Immersion of one limb causes both pupils to dilate equally, except in some few instances, where one pupil expands much more than the other, from some constitutional peculiarity, which remains the same whichever foot be immersed.

Although I have never failed to obtain dilatation of the pupils by the immersion of the foot in this solution of atropia, yet, in some cases, it takes more slowly than in others. The age of the animal has, in this respect, a most marked retarding influence. On animals only about a third grown, it will often occur at about two and a half minutes after immersion, while in the adult it generally requires five minutes and upwards.

The local effects of immersion are redness, heat, and swelling of the foot, accompanied sometimes with extravasation from some smaller vessels, when the immersion has been prolonged for ten minutes and upwards. The sensibility of the part is likewise diminished, but in no case so as to produce insensibility. The amount of irritation is of course variable, according to the duration of the immersion. It is, however, important to remark that full dilatation of the pupils may be obtained without any symptoms beyond those of a temporary active vascularization of the part, which quickly disappears when the irritating cause is removed, and which presents no more active symptoms than those produced by neuro-paralysis of the vessels after section of the sciatic nerve.

If instead of immersing the limb as above, we merely plunge it for a moment in the solution, we likewise may have dilatation of the pupil, but more slowly.

The same effects are obtained even although the limb be washed on its withdrawal from the solution, which would lead to the inference that the effect in that case is owing to absorption of the atropia, at all events beneath the cuticle.

In the case of a solution of atropia in turpentine, a still more curious effect is observed, viz., that during immersion in the liquid the pupil scarcely, if at all, dilates; whereas, immediately after the removal of the limb, the dilatation commences. Dilatation of the pupils will generally persist from twenty-four to thirty-six hours, and the return to the normal size is very gradual. In some cases the pupil may be affected after an immersion of nine minutes, the dilatation reaching three millimetres; while in others, only a very slight influence is obtained on the pupil after an immersion of from twelve to fifteen minutes. If the limb is then removed from the solution, the pupil dilates to its maximum in a few minutes. After two or three minutes' immersion, the animal shows signs of considerable pain. Much inflammation of the part follows the action of this solution, which is followed by œdema.

When we immerse the tail of the animal instead of its foot, absorption takes place much more slowly, dilatation of the pupil being produced only after the lapse of about twenty minutes.

Atropia and Alcohol.—If we substitute alcohol instead of chloroform as a solvent, we find that absorption is extremely slow. Instead of obtaining dilatation of the pupils in two or three minutes, we find that an immersion of twenty to thirty minutes in the alcoholic solution will only produce very slight effects. At the same time the local irritation is much less than that caused by chloroform. Alcohol of various strength, from proof spirits upwards, had the same result as a solvent.

Atropia in Water, with the addition of sufficient Acetic Acid for its solution.—The absorption of atropia in this state is very slow, thirty minutes' immersion frequently producing no dilatation of the pupils. Dilatation is then promoted by removal of the limb from the solution.

Watery Extract of Belladonna.—When rubbed over the leg and tail, this substance was not found, after the lapse of an hour, to produce any dilatation of the pupil.

Tincture of belladonna, with half its quantity of chloroform, produced dilatation at the end of fifteen minutes. The part was found on removal to be completely insensible, and considerably swollen from œdema, which lasted for several days.

Atropia with strong alcohol and ammonia produced dilatation of the pupil after twenty-five minutes' immersion. In this case, the ammonia was added for the purpose of ascertaining how far irritation of the part was conducive to absorption. Slight vesication was the consequence of the presence of ammonia. The acceleration of the absorption was very slight, as the solution produced no dilatation until after twenty-four minutes' immersion.

Absorption of Morphia.—The foot of a young rat at one-third its growth was immersed in a solution consisting of half a grain of acetate of morphia in twenty drops of alcohol and one drachm of chloroform. In five minutes the pupils gradually dilated to the maximum; the limb was then withdrawn; foot hot, red, and rather swollen. Irritation of the skin caused no cry, the animal merely withdrawing the part. Somnolency existed, from which any noise aroused it, but only for a moment. When placed on its back, the animal remained in that position. Respiration accelerated. Vision when roused very imperfect, as was shown by its falling off the table. The pupils continued fully dilated, the iris being reduced to an almost imperceptible circle, the dilatation exceeding that which I have been able to attain even with atropia. I will not dwell more fully at present on this last interesting fact, which is opposed to what we generally meet with in the administration of morphia. Twelve hours after, pupils normal, animal quite well.

Strychnia and Chloroform.—After three minutes' immersion of foot, dilatation of pupils ensued. After five minutes, the immersed limb was very sensitive, apparently more so than normal. Limb removed from solution: spasms about

the throat now appeared, which were rapidly succeeded by stiffness of the trunk, increasing into tetanic spasms. Death two minutes after removal.

Strychnia and Alcohol.—Foot immersed in a solution of alcohol and strychnia for upwards of thirty-five minutes; no symptoms of strychnine poisoning. Removed from solution and washed. Twelve hours later, no dilatation nor contraction of pupils.

The above observations evidently show that medicinal substances may be very rapidly absorbed into the circulation under certain circumstances, among which, the most important is the choice of the menstruum in which they are dissolved.

It remains for us to examine into the effect of temperature, inflammation, neuro-vascular paralysis, &c., on absorption. But, what is of still more importance, we have to see how far these facts are applicable to man in health and disease.

Meanwhile, I take this opportunity to state that a remarkable uniformity exists between cutaneous absorption in man and in the lower animals, and I believe that the application of these facts to practical medicine promises to be very important and extensive.—*Journal of the Royal Society.*

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2. *Subarachnoidean Fluid.*—M. JOBERT DE LAMBALLE publishes the following singular case, to prove that Magendie erroneously considered the loss of the subarachnoidean fluid a cause of functional disturbance to the cerebro-spinal system.

A person of strong constitution, who was admitted into the Hôtel Dieu on the 11th of December, 1858, and died eleven days later, on the 22d, had been stabbed with a dagger by a man who was in the habit of paying her frequent visits. The blow was struck with great violence, and the blade had broken off, close to the handle of the poniard, the base corresponding to the skin, while its point penetrated into the vertebral cavity. The large bloodvessels had escaped injury, and the hemorrhage was therefore unimportant; but a continuous discharge of a serous fluid, analogous to the serum of blood, was observed oozing from the oblique wound of the integument. The sheets and bedding were wet through, so great was the amount which exuded daily, and the fluid, on examination, was found to consist of serum containing a few blood-corpuscles. It was impossible to remove the foreign body before the third day of the patient's sojourn in the hospital, and when it was withdrawn, a considerable rush of the same liquid took place. No muscular collapse, or diminution of muscular contractility was observed at any time, notwithstanding the escape of the cerebro-spinal fluid, nor any change in the mental powers. The patient having died from spinal meningitis, a post-mortem examination showed that the body of the sixth and seventh cervical vertebræ had been grazed by the instrument, the inter-vertebral substance injured, and the parietal dura mater and arachnoid perforated by the point of the blade.

The case is favourable to M. Longet's view, who, repeating Magendie's experiments, demonstrated that the withdrawal of the cerebro-spinal fluid does not materially affect the gait of animals.—*Journal of Pract. Med. and Surg.*

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3. *Presence of Urea in Chyle and Lymph.*—Two years ago, M. AD. WURTZ finding at Alfort a carnivorous bull in which a fistula of the thoracic duct had been established, it occurred to him to search for urea in the chyle of this animal. To this inquiry he was induced by the consideration, that urea ought to originate, not in the capillary bloodvessels, as has been sometimes pretended, but in the intimate structure of all the tissues, at every part where materials which have become unfit for life require to be removed by the respiratory combustion. If this be the case, we ought to detect urea not only in the blood, where its presence has been ascertained for a long time past, but also in lymph, and, consequently, in the chyle of the thoracic duct. It appears, indeed, natural, that the lymphatics should contribute their share towards the absorption of the materials proceeding from the metamorphoses of the tissues, in which the radicles of these vessels are distributed. The chyle of the bull referred to was accordingly found very rich in urea. M. Wurtz coagulated, at a warm temperature, about 600 grammes of the chyle, evaporated the filtered liquid, treated it with absolute

alcohol, evaporated and then exhausted the alcoholic extract by means of ether; the latter deposited perfectly colourless crystals of urea, which was partially converted into nitrate. This result induced M. Wurtz to extend his researches to lymph. Having procured, through the kindness of M. Colin, the lymph of the dog, the cow, the bull, and the horse, he ascertained in them the presence of urea. It further appeared of interest to compare the quantities of urea contained in the blood, chyle, and lymph of the same animal. For this purpose it was necessary to undertake certain quantitative researches, which were accomplished by means of a process which it would be too long to explain, but which is essentially founded on a combination of the methods which MM. Liebig and Bunsen have proposed for the determination of urea. The following table presents the numerical results obtained in these researches:—

Name of Animal.	Diet.	Quantity of Urea contained in 1000 grammes of		
		Blood.	Chyle.	Lymph.
Doe	Flesh	0.089	...	0.158
"	"	...	0.183	...
Cow	Dried lucern	0.192	0.192	0.193
Bull	Lucern and rape-cake . . .	...	0.189	0.213
"	Oil-cake before rumination . . .	...	...	0.215
Ram	Ordinary diet—rumination suspended	(arterial) 0.248	0.280	...
Sheep	" " "	...	0.071	...
Horse	" " "	...	...	0.126 0.112

M. Wurtz adds, that having had occasion to analyze a certain quantity of chyle properly so called, collected along the course of the mesenteric chyliferous vessels, after the ganglions, he ascertained there also the presence of a small quantity of urea. This is no doubt derived from the changes of tissue which are effected in the walls of the intestine itself.—*Edin. Med. Journ.*, Dec., 1859, from *Comptes Rendus de l'Acad. des Sciences*, July, 1859.

MATERIA MEDICA AND PHARMACY.

4. *Saccharated Lime.*—Lime dissolves in water in much larger proportion in presence of sugar, and this solution is strongly recommended as a tonic and antacid by Dr. John Cleland (*Edinburgh Med. Journ.*, August, 1859).

Dr. C. gives the following formula for its preparation:—

"Slake 8 ounces of quicklime; rub up with it 5 ounces of white sugar; add 1 pint of water; stir for some time, till the hard stiff masses which the sugar and lime are liable to run into are as much as possible dissolved; then filter. The product should be perfectly clear, and of only a slightly yellowish tint. A solution made in this way will contain 18 grains of lime in every ounce by weight, and altogether about 106 grains of solid matter to the ounce. Taken undiluted, a few drops are sufficient to roughen the tongue. When diluted, the taste is at first an acrid one of lime; but this is immediately replaced by a sweet taste in the back of the mouth, admitted to be pleasant. Made as just recommended, the solution is not liable to decomposition unless it is exposed to the air. By employing a smaller proportion of water to lime, a still stronger solution may be obtained, but not with any practical advantage, as there is increased difficulty of filtration and greater tendency to decomposition."

This preparation, Dr. C. states, "is, of course, a powerful antacid, and probably the best we have, since it is stronger and pleasanter than magnesia, and does

not weaken digestion like the alkalies. Far from doing so, its most important use is as a tonic of the alimentary system in cases of obstinate dyspepsia. As such, its action is much more powerful than that of the vegetable stomachic tonics. It is suitable for cases with too little as well as for those with too great secretion of gastric juice, no doubt because the former state of matters is obviously a result of atony, which the lime removes, It seems particularly serviceable in gouty constitutions. In the dyspepsia of hysterical and anemic cases it does not seem to be of great use. Care should be taken to tell the patient not to take it before breakfast, as it sometimes causes a degree of nausea in the morning, when the stomach is empty. It suits very well to take it immediately after meals; its alkalinity does not at all interfere with digestion. Practitioners seem generally to take up the prejudice beforehand, that saccharated lime must be liable to produce constipation, probably judging so from the action of chalk; but I wish particularly to insist that it has not, in the slightest degree, any tendency to occasion such an effect. On the contrary, it is a very valuable means of overcoming gradually that chronic constipation which is so frequent an accompaniment of dyspepsia; and persons who have for years been in the constant habit of using aperient medicines have been able to abandon them in great measure after taking this remedy for some time. In a single instance it acted as a purgative, so that its use could not be continued. It will be found serviceable in checking the diarrhœa of disordered digestion, acting as lime-water does, only that the latter is so dilute that it is often impossible to administer it to adults in the quantity desirable. Patients who take saccharated lime habitually get to like the taste, and seem to think it exhilarating. It may be found useful also in allaying the cravings of the intemperate. I have no doubt that, if it be fairly tried, practitioners will find it an exceedingly useful remedy. It may be given in doses of from 20 or 30 to 60 minims or more, in a glass of water, two or three times a day."

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5. *Use and Properties of Perchloride of Iron.*—The solution of this persalt is now almost universally employed to arrest arterial or venous hemorrhage, resulting either from accident, or as a consequence of surgical operations. It has also been found useful in intestinal hemorrhage; in one case in particular, M. Demarquay, of Paris, administered, morning and evening, enemata of seven ounces of fluid, with twenty drops of the concentrated solution of perchloride of iron, and a tablespoonful of the perchloride syrup (five or six drops to the tablespoonful), where the hemorrhage from the bowels was considerable, and had resisted the ordinary remedies. The result was extremely satisfactory. The same surgeon relates a second case of extensive abscess of the shoulder, where an injection of iodine caused severe hemorrhage. This was arrested by throwing into the sac a lotion composed of seven ounces of water and ten drops of the perchloride.

In gonorrhœa and leucorrhœa, injections of the perchloride have been tried with success in weak and lymphatic subjects, the proportion of the perchloride being twenty drops to three ounces and a half of water.

As a *Hæmostatic.*—1. As a local or external hæmostatic, 3 to 5 parts chloride of iron to 100 parts of distilled water. Lint soaked in this mixture is to be applied with more or less pressure on the seat of hemorrhage. 2. As an external hæmostatic, 1 part of chloride of iron to 500 of distilled water, sweetened to taste. One tablespoonful to be given every hour, or oftener, if necessary. This formula suffices to check the fiercest hemorrhage within twenty-four hours. The same formula, without sugar, forms a useful uterine injection or astringent lavement in cholera or colliquative diarrhœa. 3. A hæmostatic and resolvent ointment is composed of 4 to 15 parts of chloride of iron to 30 of axunge.

In a letter in the *Medical Gazette*, August 27th, Mr. J. Zachariah Laurence states that having, a few months ago, drawn the attention of the profession to the powerful local styptic properties of the *solid* perchloride of iron, he has since that time found a superior method of employing it. "If the solid perchloride of iron be kept in a bottle, a small portion of it after a time deliquesces into a thick brown fluid, which is constantly kept in a state of super-saturation

by the undeliquesced portions of the salt. This liquid, applied by means of a spun-glass brush to a bleeding surface, arrests the bleeding almost instantaneously. This mode of application is particularly valuable in applying the styptic to such cases as excision of the tonsils, bleeding from the deeper-seated gums, &c."—*Pharmaceutical Journal*, Oct., 1859.

6. *Alum Lozenges in Affections of the Throat.*—M. Argenti, of Venice, proposes, as a substitute for alum gargles in affections of the throat, lozenges formed of alum, sugar, and tragacanth, mixed up with diluted laurel-water, so as to form lozenges, each containing a suitable dose of alum. The mass is to be well manipulated, and, after division, to be put on a sheet of paper and dried by a gentle heat. The lozenges keep well, and form an agreeable medicament, which, by aid of the saliva, becomes effectually applied to the parts. A pharmacien of Paris has for some time past prepared chlorate of potass in the same manner.—*Med. Times and Gaz.*, Dec. 3, 1859, from *Bull. de Thérap.*, tome lvii.

7. *New Method of applying Chloride of Zinc.*—Dr. G. W. Spence recommends (*Lancet*, Oct. 29, 1854) the following formula for the preparation of a paste or magma of chloride of zinc:—

"Dissolve fifty grains of prepared chalk in two drachms (by measure) of commercial muriatic acid; dissolve a hundred and fifty grains of sulphate of zinc in two fluidrachms of boiling water. When required for use, mix the two solutions, and the result will be a paste weighing nearly an ounce, and containing about one-sixth of pure chloride of zinc. The proportions are nearly, but not exactly, those indicated by the atomic weights. A little study would easily produce a paste of harder or softer consistency."

8. *A New Disinfectant for Dressing Putrid Sores and Ulcers.*—Considerable discussion has recently taken place in the French Academy respecting a new preparation, introduced by MM. Demeaux and Corne, for dressing and disinfecting putrid sores and ulcers. It consists of a mixture of 100 parts of commercial plaster of Paris in very fine powder, and from one to three parts of coal tar. This mixture forms a powder of a more or less grayish colour, and a slightly bituminous odour. For application, it may also be made into a paste with olive oil, which binds the powder together without destroying its absorptive power. The following are the properties of this substance, as described by the above gentlemen: A gangrenous sore, with an abundant fetid suppuration, treated with this dressing, is immediately freed from all disagreeable odour, and the bandages, even after 24 or 36 hours, exhale no more odour than if taken from a simple fracture. An ulcerated cancer producing a fetid serous suppuration, dressed with this substance, is entirely deprived of odour as long as the dressing remains on. So also the linen saturated with pus, cataplasms impregnated with the suppuration, &c., placed in contact with this substance lose all their disagreeable odour; the infectious liquid produced by gangrene, clots of decomposed blood, tissues in a state of advanced putrefaction, treated with this substance, are immediately disinfected. Its action appears to be to arrest the work of decomposition; it removes the insects, and prevents the production of maggots. The consistence acquired, either by the powder alone or the paste with oil, does not cause the least pain to the patient, or harm to the sore. Its application may be indirect or direct, the latter produces no harm, but rather exercises a detersive action favourable to cicatrization. This dressing has the double power of disinfecting the pus and other morbid products, and of absorbing them; the last circumstance is of the greatest importance, because it enables the use of lint to be dispensed with. Fifty kilogrammes of this powder may be made in Paris for one franc. M. Velpeau, at the Hôpital de la Charité, and several other French surgeons have employed this preparation with great success, and speak very highly of its disinfecting properties. Mr. Crace Calvert, of Manchester, has addressed a letter to the French Academy, in reference to this subject, pointing out the great variation which exists in the composition of coal tar, and the consequent necessity for more accurately ascertaining to which of the constituents the disinfecting properties are really due, in order to insure the uniform action

of the preparation. From the results of his own experiments he considers that the antiseptic properties of the tar are entirely due to the carbolic acid present. He states, that a corpse injected with a weak solution of this acid, was preserved from decomposition for several weeks; and that a piece of flesh steeped in carbolic acid, was exposed to the weather for three years without change. He also states that a small quantity added to urine will preserve it from decomposition for some weeks: and that it is also capable of preventing the gallic fermentation from taking place in the solutions of tanning substances.—*Pharmaceutical Journal.*

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

9. *On the African Fever on the Lower Zambezi.*—The *Medical Times and Gazette* (Nov. 12, 1859) contains a highly interesting letter from Drs. LIVINGSTONE and KIRK to Sir James Clark, on this subject, the principal parts of which we proceed to lay before our readers.

"The first thing," Drs. L. and K. say, "which naturally attracted their attention as medical men was the fever, which has proved one of the greatest barriers to the advance of civilized nations into the interior of Africa.

"They enjoyed considerable facilities for observing the disease during the last twelve months, not only among those of the expedition, but likewise among the Portuguese and natives; and their experience having been very different from that of others, they deem it desirable to lay the results before our medical brethren.

"About a month was spent by the expedition in endeavouring to find an opening through the Delta, from the sea to the main river.

"This part may be described as abounding in mangrove swamps and damp plains covered with gigantic grasses. The mangrove swamps emitted a most offensive smell, and gave us the impression that they were most fitly named, 'the hot-beds of fever.'

"Above the Delta the river is remarkable for the quantity of fine sand which it carries in its waters, and deposits everywhere in banks which during about half the year are exposed to the sun; there is comparatively little mud, and in this respect as well as in the greater height of its banks which are also of sand, it differs greatly from the rivers of the west coast.

"The mountain of Meramhalla, 4,000 feet high, appears to the east of Senna, and a range of hills on the north bank of the river stretches from that village up to Lupata, above which the country is hilly, and the banks of the river rocky.

"Warned by the fate of the 'Great Niger Expedition,' it was resolved that no unnecessary delay should take place in the Delta, and that the prophylactic use of quinine should be tried.

"The season chosen for entering the river was the winter (from May to August), the most healthy time for Europeans, though not the most favourable for navigation, and during the stay of the expedition among the mangroves not a single case of fever occurred among the members, or among the officers and men of the *Pearl*, and H. M. S. *Hermes*, who accompanied it.

"Quinine was taken regularly by all the Europeans, with a single exception, to the amount of two grains in sherry wine daily; and we were quite disposed to attribute our immunity to the prophylactic so much praised for its efficacy on the Niger; although the former experience of one of us suggested its total inefficiency to ward off an attack.

"Three of our number became affected with incipient cinchonism; in their case the dose was lessened. We seemed always to be on the verge of cinchonism, as an additional dose could produce deafness and ringing in the ears to an intense degree in the course of a few hours.

"The following is the number of those who escaped without a single case of

sickness, although exposed to hard work in the sun, and frequently sleeping in the boats in the lower part of the Delta: 16 officers, 37 seamen, 12 Krumen, and 2 boys from Sierra Leone.

"Three of our number were left in charge of goods on an island about twenty miles above the mangrove swamps, but probably not beyond their influence. The soil was of stratified sand, with an alluvial layer on the surface, covered with grasses. The neighbouring country presented the same aspect as the island, and was similarly constituted. Previous to this there had been hard work and constant excitement. Those left on the island, although less exposed, had a comparatively inactive life. They had not been many days alone when two of their number became sick.

"Mr. B—— was seized with slight rigors, severe headache, delirium, with contracted pupils. These symptoms were sudden, and followed an imprudent course of exposure to the sun. Although removed in a few hours, they speedily returned after renewed exposure; but this time more severe, the full bounding pulse, dry tongue, and hot skin showing the intensity of the fever. Vomiting, which now commenced, proved the most troublesome symptom, and became an obstacle to all treatment, as remedies were not retained beyond a few seconds. Although the other symptoms abated after twelve hours, vomiting continued, and soon the disease returned in a more severe form, running on to muttering delirium, with involuntary picking of the bedclothes.

"When on the verge of coma remedies began to be retained, and the action of a large dose of calomel with jalap resin relieved the cerebral disorder, and, by reducing the irritability of the stomach, allowed quinine to be given. Convalescence was very rapid; in a few days all trace of the disease had gone.

"A fortnight afterwards Mr. B—— was again seized with the same symptoms, vomiting again being the great obstacle, and, from its continuance after the others had been subdued, rendering the convalescence slow. Loss of memory and muscular weakness continued for some time after this attack. While Mr. B—— was sick, Mr. C. L—— had fever also, but in a very different form, and we did not then recognize the two as being the same disease. In him the symptoms were those of a cold, such as we see in Europe—at first frontal headache, pain in the back and limbs, lassitude and indifference to what went on around; this state was followed by one in which he lay without speaking, headache had gone, the pulse was small and thready; the skin at one time burning hot, at others coated with a clammy perspiration. In this case slight diarrhœa had continued from the beginning. Quinine was tolerated; but although the constitutional action was evident, yet the fever did not abate until a mercurial purgative had been given.

"These were the first cases of fever among us, nor did we see more for some time afterwards.

"In the month of August all the goods had been conveyed as far as Shupanga. Two officers were left there in charge, while the steam-vessel proceeded to Tette, taking with her those who had suffered when at the island.

"At this time the Portuguese were engaged in war against a party of rebels, and the army being destitute of medical appliances of every description, an opportunity was offered of observing the fever and other diseases in their worst forms among those exposed without proper shelter or food, and in subjects frequently worn out by constitutional disease.

"The commanding officer was the first case we were requested to see: he had obstinately refused all remedies, but being then in a state of coma, and unable longer to refuse, an active purge was given, followed by thirty-grain doses of quinine, which soon restored health. In him there had been no other symptom beyond those of severe constitutional fever running on rapidly to coma. This is the type most frequent in the lower parts of the Delta, especially at Quillimane. In the vicinity of Tette it is seldom fatal. The limited experience we here had seems to indicate that the type once established has a constant tendency to recur. Should further observation confirm this, it would become of importance to send all Europeans on their arrival to the higher lands; so that, should they be subject to fever, they might have it in its milder form first, and carry that with them to the more unhealthy localities.

"While the party at Shupanga enjoyed good health, notwithstanding the partial discontinuance of quinine, several cases happened on board the steam-vessel. We may mention that the accommodation on board was both very scanty and very uncomfortable. Water running into the cabin while the vessel was under steam, so as to keep the beds constantly wet beneath; fortunately the weather was dry, or the rain would have poured in from above also, as we subsequently experienced.

"Mr. R——, the engineer, had taken quinine with unvarying regularity, had an excellent appetite, and seemed to enjoy the climate; in working the engine he was obliged to see the fire lighted at 2½ A. M., in order to have steam by 7 A. M.

"An officer had shifted his bed through the night, so as to prevent Mr. R—— getting his clothes, and he proceeded to light the fire in his shirt; the consequence was pains all over the body and limbs, the bones being especially sore, the face flushed, eyes suffused, headache, and quick pulse. The bowels had been regular, and the tongue clean. A pill composed of resin of jalap, calomel, rhubarb, and quinine, which had formerly been found efficient in fever, was given, more as a precautionary measure, than from a belief that this was a case of the complaint. It seemed more a common cold than the African fever. As soon as the remedy had operated, the symptoms abated. Quinine was then given, and one dose of ten grains produced severe cinchonism, from which, when he recovered, he was quite well without loss of strength.

"Towards the end of the rainy season the members of the expedition were divided, those who had previously suffered from fever remained at Tette, while we went down the Zambezi to the sea, and explored the River Shine, both very unhealthy districts. While one of us was exposed to sun and rain navigating the vessel, and ashore superintending the wooding, the other was engaged botanizing among grass jungles and mangrove swamps. Yet both of us escaped entirely. The use of quinine had been completely abandoned, and we are inclined to attribute our good health to the regular and active exercise which both these occupations imply. In this respect our experience corresponds with that of the Portuguese residents, who assert that while actively employed they enjoy good health.

"While we explored the high lands around Lake Shirwa the steam-vessel was left in the River Shine, under the care of quartermaster Walker. We were absent twenty-four days, Walker was seized with fever the day we left: it commenced suddenly, though he had taken quinine regularly. On our return we found that he had been delirious most of the time, and the fever had shown no signs of abating; but the action of purgatives, followed by quinine, soon restored health, and in subsequent attacks, when at the sea-coast, among the mangroves, no difficulty was experienced in cutting short the attacks at the beginning.

"Our own experience in the high lands between the Shine and Lake Shirwa during twenty-four days, when we were exposed in the early hours to the dew from the long grass, continuing the march without interruption throughout the remainder of the day over rough country under the tropical sun, and then sleeping in the open air, and yet enjoying perfect health, as did also the natives who were with us, shows clearly that there exists within a short distance of the coast a healthy region well suited for the residence of Europeans.

"This region is elevated above the sea from 3,000 to 4,000 feet, and shut off from the coast lands by the range of high mountains to the east of Lake Shirwa. It is of easy access by the Shine, which flows at the foot of the hills, and is navigable the whole distance. To the north, the southern extremity of a chain of lakes, which extend far into interior of the continent, reach within thirty miles of the River Shine. This elevated region may be looked on as the entrance, by means of these inland seas, to a great part of Central Africa, cut off hitherto from communication with European nations by the unhealthy lands which bound the coasts.

"The result of our experience has been to discontinue the daily use of quinine. It had been persevered in long after the conviction of the members had been against its prophylactic power. It is our conviction that we owe our escape from the disease far more to the good diet provided for us by H. M. government

than to the use of quinine. We have been as fully exposed to the malarious influence as any party is likely to be. The vessel in which we have had to navigate is one which takes in so much water that our beds are constantly damp, and often rotten beneath, with a quantity of water in the bilge of the ship. Yet we have found the fever quite amenable to treatment when taken early, and attention paid to any local congestions which may manifest themselves. Let it not be thought that we undervalue quinine, to it we trust for the removal of the disease when given after purgatives; in all forms of the fever we have found it of the greatest value, and three doses have always proved sufficient to induce the constitutional action even in those who have not been taking it previously.

"We have found the fever assuming a formidable type only when permitted to go on unchecked for some time in those exposed to great fatigue, damp, and poor diet, or when the irritable state of the stomach prevented the administration of quinine.

"In regard to the complications most frequently seen, among ourselves vomiting has been the most troublesome, and blistering over the stomach has seemed the best means of stopping it. We have fortunately escaped without more serious inflammatory lesions of the internal organs; but among the Portuguese two cases have been observed with pneumonia; both proved fatal—the tartrate of antimony seemed to have no effect, while stimulants were equally powerless when once sinking had commenced. Enlargements of the spleen, when of recent date, have yielded quickly to the use of the sulphate of iron and quinine.

"The ship's company, consisting of Krumen, have showed no greater immunity than the Europeens. The experiment of quinine was made with them, but its prophylactic action proved equally feeble as in our own case."

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10. *Specific Chemical and Microscopical Phenomena of Gouty Inflammation.*—Dr. A. B. Garrod, in a paper read before the British Association at its late meeting, remarked that many and discordant views were held concerning the nature of gouty inflammation, and such diversity of opinion arose from the fact, that, up to the present time, no characteristic structural change had ever been demonstrated to accompany it. The object of his communication was to supply that deficiency, and prove that special chemical and microscopial phenomena invariably attend true gouty inflammation. After alluding very briefly to the views held by the ancients, and within the last century by Murray, Forbes, and Wollaston, and by Cullen and his followers, and of the difficulties which each had to contend with in applying their hypotheses to the explanation of the various symptoms of the diseases, he proceeded to speak of his discovery of the constant presence of uric acid in the blood in gout, and his subsequent researches in the nature of that disease. From these he first drew the three following conclusions: 1. In *health*, the blood contains minute traces of urate of soda and urea, and probably of all the principles destined for excretion; but the quantities are so small, that the most careful and refined analysis is required to demonstrate their presence. 2. In *gout*, the blood is invariably rich in urate of soda, and uric acid can be readily crystallized from it. 3. In by far the greater number of diseases the blood is free from an abnormal quantity of uric acid, but in certain cases of albuminuria, lead-poisoning, and other affections, its presence can be demonstrated, and still no gouty inflammation ensue. Lastly, in many gouty subjects the same condition exists in the intervals of the paroxysms. From these conclusions, Dr. Garrod considered it evident that something more than the mere presence of urate of soda in the blood was required to produce gouty inflammation, and his next object was to ascertain its nature. For this purpose a careful examination of the joints which had suffered was required, and within the last few years many opportunities had fallen to his lot. The subjects of these examinations are divided into four classes. 1. Subjects of chronic gout with extensive chalkstones. 2. Subjects of gout with no appreciable deformity, and no visible deposits of chalkstones, except one or more specks on the external ear. 3. Subjects of gout in whom no trace of chalky matter was externally visible, and in one case only eight attacks of the disease had occurred. 4. Subjects in whom only a single joint (the ball of a great toe) had been affected with gouty inflammation, or in whom

some joint had only been once slightly inflamed. These examinations proved beyond the possibility of doubt, that in the very slightest forms of the disease, as well as the most severe, a structural change invariably occurs, and that this change, when once produced, remained, if not permanently, at least for a very lengthened time. After detailing the microscopical and chemical characters of the deposit producing this change, Dr. Garrod finished his communication by stating that he considered the facts which had been brought forward warranted him to conclude that—"Specific, chemical, and microscopical phenomena invariably accompany gouty inflammation, and these consist in the deposition of urate of soda in a crystalline form within the cartilages and ligamentous structures of the joints, and that such deposition is altogether pathognomonic, never being found in any disease other than true gout;" and again, that "Such deposition is probably the cause, rather than the effect, of the inflammatory action." Lastly, the author pointed out the great importance of ascertaining the true nature of the disease as a means of conducing to its rational and successful treatment.

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11. *A Brief Review of the Pathology of Angina Pectoris, with Cases.* By WM. B. MUSHET, M. D. It must be but seldom that a number of cases of angina pectoris are presented to one practitioner. Opportunities of autopsy, in fatal instances, must yet more rarely occur, the irregular paroxysmal character of the disorder conducing to its unfrequent appearance in hospitals and other public medical institutions, and, when encountered in private practice, the prejudices of relatives and friends for the most part opposing a barrier to inspections after death.

I therefore venture to publish the following cases, and previously to cite various eminent authorities as to the nature of the disease, the difference of opinion and statements amongst which, to my mind, leads to the irrefragable conviction, that we either know very little about its pathology, or that it is an *accidental phenomenon, independent of and superadded to* the special organic affections with which it has been observed to be associated. Dr. Heberden's description is generally regarded to be correct, as he was the first to draw attention to the disease, and his account is most usually followed, although it may be remarked that Dr. Parry and others differ from this physician as to the nature and symptoms of the malady in question.

According to Heberden (*Commentaries*, art. *Pectoris Dolor*) "There is a disorder of the breast marked with strong and peculiar symptoms . . . . not extremely rare. The seat of it, and sense of strangling and anxiety with which it is attended, may make it not improperly be called *angina pectoris*. Persons are seized whilst they are walking, more especially if it be up hill, and soon after eating, with a painful and most disagreeable sensation in the breast, which seems as if it would extinguish life . . . . but the moment they stand still all this uneasiness vanishes. . . . . Males are most liable to the disease, especially such as have passed their fiftieth year. . . . . Varieties may be met with. Some are seized standing still or sitting, or upon first waking out of sleep, and the pain sometimes reaches to the right arm, as well as to the left, and even down to the hands." Heberden saw nearly one hundred cases, one of which was in a boy of twelve. He believed it to belong to the class of spasmodic, not of inflammatory complaints. He adds, "In one case a very skilful anatomist could discover no fault in the heart, valves, or arteries, or neighbouring veins, in opening the body of one who died suddenly of the disease, except some small rudiments of ossification in the aorta."

Dr. Parry, of Bath (*Edinburgh Medical and Physical Dictionary*), differed from Heberden, and called the affection "syncope anginosa." He maintained, in the true disease, that there is neither dyspnœa nor palpitation of the heart, and that angina pectoris is a mere case of *syncope* or fainting, differing from common syncope only *in being preceded by an unusual degree of anxiety or pain in the region of the heart*, and in being *readily excited* during apparent health, by any general exertion of the muscles, more especially that of walking. He ascribed it to *ossification of the coronary arteries.*

Dr. John Wall, of Worcester, in a letter to Dr. Heberden in 1772, reminded

the latter that he (Dr. Heberden) acknowledged that he never saw any one opened who had died of this disease. Dr. Wall then proceeds to detail the appearances presented on examination of the body of a person who had died of the affection, and his statement of the symptoms and *post-mortem* inspection is remarkably exact, I deem no apology requisite for the transcription.

Dr. Wall lays great stress "on the pain under the sternum, extending on each side across the breast in the direction of the pectoral muscle, and affecting one or commonly both arms, exactly where the muscle is inserted into the os humeri." This physician met with thirteen cases, and of these ten died suddenly.

"The subject examined was sixty-six years of age, and had been six or seven years subject to the complaint. He was a short, well-made man, inclined to be fat, who, in the former part of his life, had had several very severe attacks of the rheumatism. For two or three years he only felt a very slight pain and tightness across the breast and arms, upon walking a little faster than ordinary. By slow degrees this increased, till at last he could not walk, but in a very slow pace, and with great difficulty. He could not go up stairs, nor lie down without bringing on a dyspnœa, or rather a sense of suffocation. He was free from cough till towards the latter end of his illness, and then a very troublesome one came on, attended with a hoarseness. His pulse was never irregular, but always small, and during the paroxysm it sank so much under the finger, that it could hardly be felt. He died after having struggled in the fit about two hours.

"On inspection, the surface of the pericardium was covered with fat, nearly an inch in thickness. The lungs were greatly distended with very dark blood. They were full and hard, and in the cavity of the thorax was a very considerable quantity of an aqueous fluid. Cutting into the lungs, frothy mucus, mixed with something purulent, and of a fetid smell, issued from every part, but chiefly from the divided bronchi. . . . Upon opening the pericardium, the heart appeared of an enormous size, and was covered with a great quantity of fat. The pericardium contained not less than a pint of fluid. Upon examining the heart, no part appeared diseased, till we opened the left ventricle, and there the semilunar valves placed at the origin of the aorta were found to be perfectly ossified. They did not, as usual, lie flat upon the divided orifice of the vessel, but stood erect, and appeared to be immovable. They were entirely osseous through their whole substance, but the ossification was formed unevenly, and as it were in spines, some parts being near a line in thickness, and others thin, like a connecting membrane, but perfectly bony. The aorta at its curvature was considerably enlarged, and for near an inch from the heart was in part ossified, there being several bony scales or laminæ on it, but not connected with one another.

"The origin of the disorder," says Dr. Wall, "is here evidently to be traced from the induration of the semilunar valves, which existed probably in a less degree for several years, and increased gradually till it came to a bony hardness and immobility. It is possible that this induration of the semilunar valves may not always be the cause of the disease, though it seems not improbable that some malformation in the heart, or vessels immediately proceeding from it, may be so. Indeed, when we consider how frequently such indurations in the valves of the heart have been found, that the disease in question does not come on till a person is advanced in years, and, consequently, until a rigidity in every part naturally comes on, we shall be inclined to imagine that a preternatural induration of the parts necessary to the circulation of the blood through the heart, may be the predisponent, if not the efficient cause of the disorder."

In a fatal case of Dr. Fothergill, "the only morbid appearance was a small white spot near the apex of the heart;" and in an instance recorded by Dr. Haygarth, "the heart, lungs, and pericardium were perfectly sound."

Referring to more modern authorities, we find that Laennec arranges angina pectoris under *neuralgia of the heart*. He says: "It is a spasmodic affection, which returns in paroxysms after longer or shorter intervals. . . . Angina pectoris, in a slight or middling degree, is extremely common, and exists very frequently in persons who have no organic affection of the heart or large vessels. It is certainly true that this affection frequently coincides with organic disease, but nothing proves, even then, that it depends upon such diseases, inasmuch as

they are of various kinds, and as the angina exists without any of them. Of subjects examined, who had laboured under this disease, *in none did I find the coronary arteries ossified.* One died suddenly during an attack of angina, and such a result need not surprise us, when so severe a nervous affection coexists (as in this case) with extensive hypertrophy." Dr. Hope "thought the disease to be owing to structural disease of the heart or great vessels, in which some portion of them was deprived of their elasticity by osseous, cartilaginous, or steatomatous degeneration, and that pain was dependent on overtension of the rigid portion.

Dr. John Forbes (*Cyclopædia of Practical Medicine*) seems to consider it doubtful whether angina pectoris would prove fatal in the absence of structural disease of the heart. He observes, "there is reason to believe in the few cases of fatal angina, in which no organic lesion was found on dissection, the heart was not exactly of just proportions, or of perfectly sound structure."

Dr. Wood, of Philadelphia, regards "neuralgia of the heart and angina pectoris as identical, as though angina has been frequently found in connection with organic disease of the heart, yet frequently also no such affection has been detected after death, so that it must be considered as essentially nervous."

Dr. Copland (*Dictionary of Medicine*) remarks "in several instances, not the slightest morbid appearances could be detected, but more frequently the heart and large vessels in its vicinity have presented marks of disease, generally varied in its nature, and opposite as to its characters." This author is inclined to impute the affection to a species of neuralgia of the cardiac and pulmonary nerves. Dr. Walshe is of opinion that angina pectoris "is a neurosis, although he is disposed to think that a true angina cannot exist without organic disease of the heart."

Dr. Joy (*Library of Medicine*) states "we are disposed to side with those who believe that angina pectoris, at least in its less inveterate modifications, may exist, altogether independent of structural changes. It is only in the more aggravated and prolonged cases that such alterations have been very conspicuous."

Dr. Latham says "angina pectoris has existed where no form of disease or disorganization whatever has been found, either in the heart or in the blood-vessels nearest it." He thinks it to be "immediately *a spasm of the heart.*"

I shall now narrate the following two cases, one of which proved suddenly fatal, a careful *post-mortem* examination being instituted; the other in an old lady yet surviving, but which I regard as an undoubted example of the disease:—

CASE I.—E. H., aged seventy-eight, a remarkably hale, stout old man, retaining all his functions, and employed as a messenger, had suffered for some months from pain over the region of the heart, coming on paroxysmally, and occasionally passing down the left arm, though this latter symptom was by no means of constant occurrence. During the attacks, he experienced a feeling of sinking and constriction of the chest, and a difficulty of breathing, together with considerable anxiety. The dyspnœa was not habitual in the intervals, and he had but very little cough. The pain was not periodic, but came on at irregular times. Auscultation was not practised, but he was ordered sulphuric ether and ammonia which afforded great relief. He fell dead on the stairs one evening about 8.30 P. M.

Examination of the body, eighty-four hours after death. Weather cool. The pupils were moderately dilated. The arcus was strongly marked. Rigor mortis was tolerably manifest in the lower limbs, less in the upper. There was slight œdema of the feet.

Head.—The vessels of the scalp were very turgid. The calvarium was remarkably dense. The dura mater was adherent to the skull anteriorly. The arachnoid was opaque at spots; and there was some fluid between it and the pia mater. The brain-substance was a little extra-vascular. A small amount of serum was found in the ventricles. About two ounces of sanguinolent fluid were found in the base of the skull. The sinuses at the base were filled with semifluid dark blood. The arteries at the base were atheromatous, not calcified.

Chest.—The heart was entirely adherent to the pericardium; the adhesion was firm, but could be broken down with the finger. On attempting to separate the membrane from the heart anteriorly, some of the substance of the organ was also removed. The heart felt flabby. It was elongated transversely, and there was a white patch near its base over the right part of the left ventricle, of about the size of a shilling, which had a slightly roughened feel. There was a corresponding patch on the opposed pericardium. About the apex were two or three gelatinous-looking cysts, which appeared to be beneath the serous membrane. A large quantity of clots, and a little, partly decolorized, escaped on section of the inferior cava (several ounces). There were also dark clots in the pulmonary artery and aorta, but the quantity of blood was small on the left side. The heart freed from blood weighed seventeen ounces and a half. Its structure appeared to be tolerably healthy to the eye, but fat encroached on its walls, especially at the apex, and at base of the right ventricle. Portions, from these situations, exhibited fatty degeneration under the microscope. The cavity of the right ventricle was dilated, its walls a little hypertrophied. Both auricles were dilated. The left ventricle was hypertrophied. The pulmonary valves were normal, but one segment was reticulated; they supported a column of water. The flap of the tricuspid valve next the septum was natural, but the other two were thickened and beaded with fibro-cretaceous deposit. The aortic valves appeared sound, but did not perfectly support a column of water. The mitral valve was healthy; the tricuspid orifice readily admitted four fingers; the mitral admitted all the fingers and thumb; the aorta and pulmonary artery appeared to be neither constricted nor dilated; but the former vessel displayed considerable atheromatous degeneration. The coronary arteries were pervious, and loaded with atheroma, but presented no appearance of calcification. The lungs were emphysematous and congested. There were old pleural adhesions on the right side, and slightly on left, and a few ounces of serum in the left side of the chest. No tubercle was present. The bronchial tubes were intensely injected, containing but very little secretion. There was no softening of the mucous membrane. The liver was contracted, with stellate deposits on the surface. The right kidney weighed five ounces; it was congested; the capsule separated well. The left kidney was also congested; it weighed five and a half ounces; the capsule separated, with a portion of the subjacent tissue attached. The bladder was distended. The spleen was normal. The stomach was empty, congested in patches. The intestines were also congested. The pancreas was hard and granular to the feel.

Case II.—Mrs. D., aged fifty-nine, unmarried, has usually had good health, and worked hard in service as cook and housekeeper. She first suffered from rheumatism at thirty-five. This was an acute attack, and disabled her for some months. She has since suffered at intervals from the disease, for which she was a short time ago under treatment. At irregular periods, for some years, generally when she has pains in her limbs, or is *flurried* or *excited*, though occasionally at other times, especially when out of health, she has experienced a sensation of spasm over the front of the chest, deep-seated, and inclining to the left side, and sometimes passing down the arm. "The feeling is very singular and crampy, and as if she were drawn together, and she feels a sort of stagnation, as if the heart stopped." During the attack, which is sometimes only momentary, and sometimes lasts for several minutes, she is quite conscious, and her sight does not fail. She has but very slight cough. Dyspnœa, palpitation, or globus never appears to be present, and there is no history or evidence of hysteria, though she is troubled with flatulence. Two or three attacks may occur in a month, or a year may elapse without their recurrence. They come on indifferently in the day and at night, and mostly when she is at rest. The climacteric epoch was not attended by much disturbance, but she had two or three seizures of epistaxis. Arcus is evident in both corneæ. Her pulse is regular, not intermitting or rigid. The heart-sounds are tolerably good, but feeble. Respiration is a little harsh. Pains of rheumatic character, chiefly affecting the muscles of the arms and calves, lately induced her to seek medical aid, and during her illness, the attacks described were of frequent occurrence, sometimes daily.

Ammonia, Dover's powder, iodide of potassium, quinine, etc., were prescribed,

and in a week or two she became convalescent, the anginal symptoms disappearing with regained health.

How can we harmonize the discrepancies which have obtained amongst the profession, as to the symptoms and pathology of angina pectoris, ever since the first description of the disease?

Most authors admit the necessary association of *some* cardiac affection, and that the cardiac lesion is variable to which the angina is a supplemental symptom. Others regard it to be dependent on a particular morbid agency, as calcification of the nutrient vessels, induration of the valves, spasm of the heart,[1] etc., but the difference of opinion amongst these latter, and *post-mortem* appearances, sufficiently indicate that such doctrines are untenable and erroneous.

It is moreover evident, that every conceivable morbid process in the heart and great vessels may exist, in absence of observed symptoms of angina, as atheromatous and fatty degeneration, calcification and occlusion of coronary arteries, valvular disease, adherent pericardium, hypertrophy, dilatation, atrophy, coarctation or dilatation of the aorta, etc. Conversely, can angina be present in absence of all the diseased states enumerated? In some cases, not any morbid appearances in the heart or great vessels are reported to have been noticed after death; but these statements must be accepted with reservation, when we take into account the imperfect state of pathological knowledge which until lately prevailed, and we have no authentic proof that in these cases the individuals died suddenly in the anginal paroxysm, an event necessary to confirm the accuracy of the diagnosis. Again, fatty heart, a fruitful source of sudden death, was a change unrecognized by our earlier pathologists.

It is incontestable that angina pectoris either *can* or *cannot* be developed without appreciable structural alteration in the heart or great vessels. If the former proposition be true, the disease must be considered as a true neuralgia; and I affirm that such an affection is incapable of proving fatal *per se*.[2] If the latter be true, when death results during a paroxysm, it is more reasonable to presume that it is a consequence of the organic disease than of the superadded anginal (neuralgic?) symptom, which is clearly not necessary to explain the fatal issue, as sudden death is of frequent occurrence in cases of heart disease, in which angina has never been observed. Were it present, death might be attributed to what is, I believe, merely an accidentally associated neurosis.

It may be advanced, that the aggravated symptoms and anxiety experienced by patients are incompatible with the view of the angina being an unimportant element in the affection, but it is well known how timorous and depressed individuals become when suffering from functional palpitation or pain referred to the cardiac region; consequently, when similar symptoms arise in persons labouring under organic disease of the heart or great vessels, we can readily estimate the influence exerted on an obstructed circulation through the emotional system.

It is agreed that angina pectoris seldom announces itself before forty or fifty years of age, about which period disease of the heart commences to be fatal to any extent. When occurring in children and early manhood, I conceive the affection to be simply neuralgia (equivalent to angina pectoris *minus* heart affection), and such a notion is corroborated by a cure being, for the most part, effected in this class of cases.

On the whole, therefore, I regard angina pectoris as a neuralgic affection,[3] always connected with some heart disease (in which respect it differs from simple neuralgia), being an accidental complication of the latter, and only occasionally

[1] That this is not essentially true, is proved, as in many cases the pulse remains undisturbed during the paroxysm. And we cannot imagine an attack of angina, if dependent on spasm of the heart, to continue for half an hour and not prove fatal.

[2] Is there any physician of the present day bold enough to assert from theory, or who can attest from experience, that angina pectoris has ever proved fatal in persons with a healthy heart?

[3] The acknowledged efficacy of antiperiodic remedies strongly supports this conclusion.

present, as when the sufferer is dyspeptic, out of health, or subjected to any influences which are prone to induce neuralgia in other parts of the system. Some, as Dr. Chapman, are of opinion that angina pectoris is a gouty or rheumatic affection; and it must be admitted that recorded cases tend to reveal its connection with these diatheses; and it is almost unnecessary to advert to the relationship of gout and rheumatism to neuralgia and diseased heart.

I think this group of complaints may be well illustrated by the subjoined simple diagram.

Rheumatism (Gout)
Neuralgia . . . . . Disease of Heart
Angina Pectoris.

Assuming the neuralgic origin of the disorder, it is interesting to consider the nerves implicated. That the heart is not the seat of suffering appears to be almost proved, as this organ is normally insensible, and even when seriously impaired by grave structural changes, and the varying situation of the pain (behind the sternum, on the left side, down the left arm, on the right of the thorax, at the insertion of the deltoid, at the bend of the elbow, etc.), appears inconsistent with such a presumption. A few, as Desportes, locate it in the pneumogastric nerve, but in opposition to this, the last-mentioned objections hold good. I am disposed to conclude, that the intercostal are the nerves involved, the differences presented by the symptoms in different cases being explicable by consideration of the particular nerve affected, in conjunction with the special coexistent organic affection, and reference to its anatomical connections and distribution.—*Brit. Med. Journ.*, Oct. 15, 1859.

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12. *On Diabetes.*—In an elaborate article on this disease, Prof. W. Griesinger inquires into its pathology and treatment. We extract some of his remarks in reference to the theory that the liver is the organ mainly at fault, which has been specially revived since Bernard's discoveries regarding the glycogenic function of that organ. The author denies the correctness of Andral's observation, that in diabetes there is post-mortem evidence of over-activity in the glycogenic function. In none of his cases was there any enlargement of the liver detected during life, and only in one of five cadaveric inspections was the organ found to be somewhat hypertrophied. In this case it was very convex, with a sharp edge, eleven inches and a half in the longest diameter, six inches in the transverse diameter, and the weight, with the gall-bladder, was sixty-three ounces; on the surface were a few asteroid injections; in the interior, many small, pale, dirty-yellow spots; the tissue generally was exsanguine, friable, and granular, and the gall-bladder contained much brown bile. A careful examination in the other four cases showed no anomaly of the liver. A comparison of sixty-four cadaveric inspections confirmed the author's observation that diabetes mellitus is not characterized by any peculiar morbid condition of the liver. Dr. Griesinger does not appear to have noticed any microscopic changes in the hepatic tissue, and the evidence he quotes with reference to the increase or diminution of the oily contents of the hepatic cells is contradictory; thus Frerichs states that the absence of oil is characteristic, while the reverse is affirmed by Beale; Forster, again, maintaining the tissue to be perfectly normal. It certainly is remarkable that if the liver is the organ mainly at fault in diabetes, we so rarely meet with it directly indicative of hepatic disease. Among 225 cases analyzed by the author, there were only two that exhibited hepatic symptoms, both having been preceded by icterus immediately before the appearance of saccharine urine. As an apology for pathology, Dr. Griesinger quotes some apparently contradictory statements by physiologists who have specially worked at the subject, and which it may not be uninteresting to reproduce.

"Exclusively animal diet does not diminish the quantity of sugar in the liver; exclusively amylaceous diet does not increase it." (*Bernard.*)

"With a mixed diet (partly amylaceous) there is much more sugar in the liver than with an exclusively animal diet." (*Stockvis.*)

"After an exclusively animal diet there is more sugar in the liver than after vegetable diet." (*Figuier.*)

"The liver of graminivora contains much more sugar than that of carnivora." (*Bernard.*)

"There is no material difference in the amount of sugar contained in the liver of graminivorous and carnivorous animals." (*Poiseuille and Lefort*, 1858.)

A careful examination of the evidence regarding the influence of treatment leads the author to the conclusion that, with the exception of alkalies, none of the remedies commonly used exert any directly curative effect. Nor did the exhibition of the carbonated alkalies do more than reduce the quantity of sugar secreted; they did not arrest the secretion or cure the disease. The details of these therapeutic observations are of much interest, and deserve a careful study. Still, though drugs can do little, the author is willing to admit that by avoiding injurious influences, by a suitable hygienic regimen, and by a generally tonic proceeding, much may be done to improve the patient's condition and to prolong life.—*B. and F. Med.-Chirurg. Rev.*, Oct., 1859, from *Archiv. für Physiologische Heilkunde*, Jahrg., 1859.

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13. *Two Essentially Distinct Conditions of the Kidney giving Rise to what is called Bright's Disease.* By Mr. DICKINSON. We extract the following from the *British Medical Journal*:—

The results of the author's minute examination of the kidney under disease, lead him to the conclusion that the conditions, called after the name of Dr. Bright, are capable of arrangement into two great classes; that in the one we have disease proper to the secreting surface of the tubes; while in the other the disease is essentially intertubular, and that the characteristic difference between the two is the presence in the latter, and not in the former, of external granulation.

The "tubular disease" includes all the varieties of the large smooth kidney; notwithstanding that pathologists have subdivided it, according to the predominance of some one material, into "fatty degeneration," "waxy degeneration," "desquamative nephritis," and many others. There are two distinct stages. First, the period of enlargement; secondly, that of diminution. In the first stage, the capsule is thin and unadherent; the cortex is much, the cones slightly, enlarged; there is no appearance of preternatural vascularity. Parts, indeed, may be bloodless, giving a mottled aspect. The microscope shows the tubes entire; but in the cortex they are choked up with their epithelial secretion, which is often in various stages of disintegration, or fatty degeneration. These are probably consequences rather than causes of the derangement, and result from the cells, formed in excess from the inflamed membrane, being of unhealthy composition. In the cones there is the same tendency to excess of epithelial secretion, but it finds a readier exit, and does not, therefore, so often accumulate there. The tubes here show rather a tendency to become filled with a glassy fibrinous material, studded with specks of oil, or cells of epithelium, which happen to be passing down, or blood corpuscles; and this is shed in the form of cylindrical casts. The casts, therefore, are formed almost exclusively in the straight tubes.

The second stage is rarely reached. It consists in an over-stretching and bursting of the tubes one after another, those nearest the superficies being first destroyed; and it appears that their contents are then absorbed. The tubes collapse, and the organ may be reduced below its natural size. The capsule becomes thickened and a little more adherent. The surface is still smooth, the colour unchanged. A section shows the gland to be chiefly composed of cone substance, and the remaining cortical substance is coarse, pale, and firm in texture, being reduced more or less to its fibrous nature; but there is no evidence of an increase of fibrous matter. The author agrees with Mr. Simon in believing that the cysts, often present in this condition, are developed out of epithelial germs, which are left on the walls of the ruptured tubules.

The "intertubular," or "granular disease," commences with slight thickening and adhesion of the capsule, with processes of fibrous-looking material passing from it into the interior of the gland; contraction of these follows, the points at which they originate become depressed, giving a superficial granulation to the

exterior; and the tubes embedded in them, at first healthy, become gradually reduced to microscopic threads.

In what the author calls "chronic intertubular disease," there is generally diminution of size. The adherent capsule has a tendency to split into two layers; the inner one consisting apparently of fibrinous effusion. The surface is granulated, and fibrous processes pass from the capsule between the granules. The fibrous tissue of the cortical part is increased, and the tubulous waste. Some of the fibrinous material passes into the interior of the tubules, lining them and displacing the epithelium. Cysts are often present, and evidently result from transformation of the tubes, not, as in the tubular disease, from a change in the epithelium.

In the "acute intertubular disease," the granules are larger and more distinct, and there is more fibrinous exudation between and into the tubules.

The anatomical evidence in favour of the intertubular origin of the disease may be summed up as follows:—

1. Near the capsule an effusion is often seen obviously between the tubules.

2. Tubes are caused to shrivel by some influence which travels independently of their direction.

3. Tubes are sometimes divided into cysts, which can only be accomplished by a contractile force, external to themselves. In the smooth kidney this never occurs.

4. In most cases, some of the secreting tubes are found natural, as if they had been out of the way of the effusions; whereas, in the smooth kidney all are involved.

5. A decided difference exists in the contents of the tubes in well-marked cases of each sort. In one they are filled by their own morbid secretion, in the other they are occupied by a material which is foreign to it, and which is probably derived from without.

Of 357 fatal cases in St. George's Hospital, of which the particulars were collected by the author, 250 were granular, 107 smooth; of the latter the average age was 28 years, of the former 50 years; proving that the one disease is not a sequal or stage of the other. In no single instance has he known or read of the granular form resulting from scarlatina; and it rarely results from cold. Gouty or rheumatic diathesis was associated with the granular form in seventeen cases, with the smooth variety in only one, and that doubtful.

In examining the kidney, besides the ordinary mode of tearing the organ into shreds and fibres, it is the author's plan to prepare translucent sections, so as to show the tubes and Malpighian bodies in their relations to one another, and to the intertubular material.

A portion of the kidney is boiled in water, slightly acidulated with acetic acid. It is dried, and when hard a section can easily be cut from it, which, on the application of water, resumes its former bulk, and presents a beautiful picture of the anatomy of the kidney.

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14. *On certain Points connected with the Treatment of Renal Anasarca.*—Dr. Fuller read a paper on this subject before the Western Medical and Surgical Society. The points the author selected were—first, the cause of the epileptiform seizures and other head symptoms which arise during the progress of renal disease, together with the best mode of obviating them; secondly, the propriety of employing ferruginous preparations and renal stimulants in certain stages and in certain forms of renal disorders. Dr. Fuller observed, in regard to the first point, that although the head symptoms are commonly supposed to be due to the non-excretion of urea by the kidneys, and its consequent accumulation in the blood, many facts conduce to the belief that the mere existence of urea in the blood is quite inadequate to their production. Amongst the facts cited in proof of this position may be mentioned the frequency of head symptoms in cases of acute renal mischief, attended during life by extremely albuminous and even bloody urine, and determined after death by the large, smooth, mottled kidney; cases in which the urine is seldom of very low specific gravity, and in which a considerable amount of urea is excreted; and, on the other hand, the great length of time, the long series of years, in which patients suffering from

the small, dwindled, granular kidney, whose urine ranges from 1002 to 1008, will go on with their intellects unimpaired and their nervous systems undisturbed, in spite of the extreme scantiness of the excretion of urea. Dr. Fuller mentioned the case of one such patient, whom he had had under observation at intervals during the last fourteen years, the specific gravity of whose urine throughout that period had never exceeded 1007, and usually averaged 1004. The urine was not particularly abundant; the quantity of urea excreted was very small; yet the man, though languid, and unequal to long-continued bodily exertion, had never suffered in the slightest degree from disturbance of his sensorial functions.

Another fact, seeming to point to some other sudden and temporary alteration in the condition of the blood, some decomposition of the urea long existing in the blood, or some other unusual chemical change, as the cause of the convulsions and insensibility, is the suddenness with which these symptoms arise, and the speed with which they pass off. Until within a few hours before the attack there is usually nothing in the general appearance of the patient, or in the condition of his urine, to indicate a change for the worse; and when the symptoms pass off, which they do in a short space of time, there is no observable improvement in the general symptoms, no appreciable alteration in the condition of the urine. Nevertheless, the author drew attention to the fact that head symptoms are specially prone to arise when the patient is unduly lowered or depressed, and he urged the necessity of extreme jealousy in maintaining as healthy a condition of blood as possible, by regulating the supply of food, by enforcing exercise, encouraging the action of the skin, and in every way providing for the elimination of the effete materials of the body by a more than ordinary demand on the activity of the excretory organs; and, further, in avoiding all measures, whether dietetic, medicinal, or otherwise, calculated to exhaust or unduly depress the nervous centres. With a view to guard against this condition, he urged the expediency of maintaining a proper tone of the system by the constant use of diffusible stimulants, and by the occasional administration, for a lengthened period, of small doses of iron in a light bitter infusion.

In respect to the administration of direct renal stimulants, Dr. Fuller combated the opinion so often propounded, that when the kidneys are diseased our aim should be to avoid stimulating them, and to prescribe such remedies only as will increase the action of the other excretory organs. Observation, he said, had long since convinced him that the practice founded on this opinion is extremely inefficacious: and he maintained that the use of vapour-baths, diaphoretics, and purgatives, to the exclusion of diuretics and direct renal stimulants, is as false in theory as it is unsatisfactory in practice. He drew attention to the fact that Bright's disease is confined to persons in weakened health, and is essentially a disease of mal-nutrition, attended by general anæmia. Therefore, on the same principle as that on which stimulating applications are employed to relieve congested conditions of the throat, languid ulcers, scrofulous eyelids, and other seats of diseased action characterized by low congestion and mal-nutrition, he insisted that in these cases of renal mal-nutrition and passive congestion renal stimulants, such as cantharides and turpentine, should be employed in conjunction with saline purgatives, vegetable bitters, iron, and whatever may be necessary to improve the general health. He acknowledged that in cases of renal mischief, evidenced by acute desquamation of the uriniferous tubes, the propriety of such treatment admitted of doubt, and that the warm bath, dry cupping on the loins, and the exhibition of digitalis and brisk purgatives, generally prove more serviceable; but he cited cases to prove that even when desquamation is proceeding actively, as it does in most instances of scarlatinal dropsy, long after the feverish symptoms have subsided, the exhibition of iron and cantharides is often followed by the happiest results.—*Lancet*, Dec. 10, 1859.

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15. *Pathology and Treatment of Idiopathic Peripheral Neuralgia.*—Mr. H. LOBB read a paper on this subject before the Harveian Society (Dec. 1, 1859). He commenced by dividing neuralgia into—1. Central; 2. Peripheral; and 3. Reflected. Of the central, arising from disease of the brain or spinal cord, he did not speak. The third division he proposed reserving for a future communication. The second was divided into idiopathic, traumatic, and neuromatous. Idiopathic

peripheral neuralgia was described as a stabbing, darting pain referred to the course of a nerve, shooting down the nerve like lightning, coming on suddenly, lasting but a moment, and repeated at intervals; the pain is so acute as to be unendurable if continuous. The part or limb affected with this form of neuralgia is colder, and the skin supplied by the neuralgic nerve is more or less numb, not tender to the touch, the patient liking to be rubbed, and frequently grasping the part with the hands and pressing it. If the neuralgia has lasted any time, there is more or less paralysis in the muscles supplied by the accompanying motor nerve. The author then gave, at some length, his views upon the generation and distribution of nerve-force from the capillary circulation, in order to make his description of the pathology of this form of neuralgia intelligible. He described a sentient nerve as a conductor to the brain of sensations taking place at the periphery; neuralgia is not, therefore, a hyperæsthesia of its healthy function. During health a nerve has no sensation proper in itself; and if it be struck, cut, or torn, the sensation is referred to the parts to which it is distributed; but in this form of neuralgia it is the nerve itself to which the pain is referred as darting up and down its course. A sentient nerve was likened to an iron conducting wire of a galvanic battery, which, if of a certain uniform diameter, conducts a given quantity of galvanism without being perceptibly affected by its passage; but if a portion of the wire be much finer than the rest, it becomes red-hot, being unable to conduct the whole of the galvanism, the remainder correlating into heat. So in a nerve. From mal-nutrition, it is unable to conduct normal sensations to the brain; the nerve-current, by affecting the polarity of the nerve itself, gives the idea of pain in that portion of the trunk of the nerve, its peripheric terminations at the same time being numbed. The author considered the indications for treatment, therefore, to be increased and healthy circulation, arterial and nervous. After enumerating the usual methods of treatment, he considered the only agent capable of carrying out these indications to be the continuous galvanic current; and he recommended, as the only apparatus that can be adapted to the surface of the body, and capable of generating a sustained current, the Pulvermacher chain—producing a continuous current of galvanic electricity in one uniform direction, mild, yet sufficiently energetic for medical purposes. Mr. Lobb then described the method of applying the chain, and exhibited some experiments with its aid—as the decomposition of water, contraction of muscles, &c. In the treatment of idiopathic peripheral neuralgia he looked upon the Pulvermacher chain as a specific. Immediately upon adapting the excited chain to the part, a genial glow is felt—not mere warmth, but a sense of vitality in the part; the patient is aware of a life-giving agent, and immediately says that he is relieved; the neuralgia disappears, and sensitiveness of the surface returns. The paper concluded with several interesting cases in which the continuous galvanic current afforded rapid and permanent relief.—*Lancet*, Dec. 10, 1859.

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16. *On the Causes of the Independence of Bronchitis in relation to Pneumonia.*—M. Robin states (*Gaz. Méd.*, No. 2, 1859) that he has never seen the causes of the independence of bronchitis in relation to pneumonia stated by any author, and this, he believes, arises from the faulty notions which prevail as to the elementary structure of the organ of respiration. It is customary to represent the tissue of the lungs as a mere continuation or expansion of the bronchi, which is as incorrect as it would be to represent the uriniferous tubes of the kidney as a continuation of the urethra, bladder, and ureter. As long as he believed in this doctrine, M. Robin never could comprehend why bronchitis should not constantly be passing into pneumonia. Nothing, however, can be more distinct than the pathological anatomy and symptoms of the two affections, which may be sometimes observed co-existing, but never passing from the one into the other.

The differences between the two diseases, marked as they are, must remain incomprehensible to those who consider the entire tube as lined with an uninterrupted mucous membrane from the larynx to the extreme subdivisions into *cul-de-sacs*. The real state of things is, however, as follows: Having passed through a certain number of subdivisions, the bronchi, now no more than one

or two millimetres in diameter, lose their portions of the cartilaginous rings, and have no longer transverse muscular fibres, elastic longitudinal fibres, or a mucous membrane separable from the bronchial wall properly so called. They no longer possess a prismatic epithelium with vibratile cilia—losing, in fact, all the characters of bronchi. The pulmonary or respiratory canalicules, erroneously termed ultimate bronchial ramifications, continue to subdivide and terminate in rounded or ovoid *cul-de-sacs* (improperly called bronchial or pulmonary cells), which at the period of birth are from five to eight hundredths of a millemetre large, and in the adult attain the size of one or two tenths. These canals have not the structure of the bronchi, but are characteristic of the pulmonary parenchyma. They are surrounded by intimately interlaced bundles of elastic fibres, mingled with fibres of the laminated tissue, formed of fibro-plastic elements, and of vessels. These vessels form on the interior of the canalicules (which present slightly projecting folds), a network differing from that of the bronchi. This network consists of large capillaries, which nearly touch each other, so as to leave intervals smaller than the capillaries themselves. It is distributed on the very tissue of the walls of the pulmonary canalicules (there being no mucous membrane separable from elastic parenchyma), and is only separated from the cavity of these conduits by a layer of pavement epithelium with large nuclei, which commences where the cylindrical epithelium of the bronchi ceases. Thus the pulmonary canals, in which hæmatosis is accomplished, have a different structure to that of the bronchi which convey the air necessary for respiration. It is not possible to detach a mucous membrane distinct from the pulmonary parenchyma and the laminated tissue, in which, or on the surface of which, the capillary network is distributed, as is the case in the bronchi still provided with cartilages. In this way, we may explain the rapid absorption which takes place in the lung, as compared with the slower absorbing power of the organs provided with mucous membrane—as also the easier rupture of these capillaries, with discharge of blood, or of substances injected by the air-passages. There is, in fact, as great a difference in texture between the bronchi and the pulmonary parenchyma, as between that of the excretory duct of a gland, and of the gland itself.

It will, therefore, be seen that affections seated in two portions of the apparatus so different, may well present great distinctions in their course, etc. But a still more important cause also explains the rarity of the extension of inflammation from the bronchi to the pulmonary tissue. Thus, in the case of bronchitis, the portion of the capillary system which is the seat of inflammation belongs to the general capillary system properly so called, and receives its blood from the aortic or red-blood system; but in the case of pneumonia, the capillaries of the lesser circulation, deriving their supply from the black blood of the pulmonary artery, are in question. It is at the expense of this black blood that the morbid products of pneumonia are formed, as in hepatitis it is at the expense of the black blood of the vena porta that abscess of the liver is produced. We know, in fact, that although the pulmonary artery accompanies the bronchi throughout their entire extent, it gives no branch to them, nor to the interlobular partitions, and that it does not anastomose with the bronchial arteries. The latter entirely cease at the points where, or at a little beyond where, the small cartilaginous nuclei disappear from the bronchi, *i. e.* where the bronchial canalicules are only one millimetre, or a little more in diameter. This is the exact spot where the capillary distribution of the venous artery begins to take place between the contiguous walls of the pulmonary canalicules, forming on their sub-epithelial surface a network of quite a special type of mesh-work, which is also found in the lesser circulation of all classes of vertebrate animals, even to the branchial plates of fishes. Beyond the bronchi, the bronchial arteries only furnish *vasa vasorum*, and branches to the interlobular laminated tissue, which extend as far as the pleura.

These circumstances supply not only an answer to the question proposed in this article; but also explain some of the differences which distinguish the nature and progress of inflammation of the lungs from that of other parenchymatous organs. It explains also the differences of pneumonia, according to age, differences not exhibited so decidedly in the inflammations of any other organ, and which arise, not only because the parenchyma and the respiratory

canalicules undergo notable modifications, but also because modifications in its nature and course are produced upon the inflammation by the nature of the circulation. These are nowhere so decided as in the lesser circulation, which unites anatomically and physiologically the two sides of the heart, although its disturbances are often only caused indirectly, in consequence of lesions of the left side of the heart, instead of directly by changes on the right side.

Independently of the special type of distribution presented by the pulmonary capillaries, differing from that of the bronchial, their structure also differs in some points from that of the general capillaries. They are, in fact, amongst the largest of the body, and their parietes present smaller, more numerous, and more approximated nuclei than those of the other capillaries. It is, however, to be observed that the capillaries of the portal system in the liver present the same peculiarities of structure. These facts are not without their value, when we call to mind that inflammation is a disturbance of the capillary circulation.

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17. *Pain as a Sign of Disease of the Stomach.*—Dr. Habershon read before the Medical Society of London (Nov. 14, 1859) a paper on this subject. He first noticed the general absence of pain in disease of the mucous membranes, except where the orifices of their canals were affected; and of the very frequent immunity from actual suffering in many morbid states of parenchymatous viscera. But in serous membranes an opposite condition was found to occur, almost any change, and especially those of a sudden or acute kind, being accompanied by severe and agonizing pain; and of such a character and severity as to demand perfect rest, this rest being a very essential element in the alleviation of the disease. In pericarditis, on the contrary, when occurring without pleurisy, pain was very frequently entirely absent, as for many years shown by Dr. Addison; and rest in this case would be impossible. The immediate object of the communication being the consideration of *pain* in connection with *disease of the stomach*, he proceeded to show its value as an indication, or non-indication, of disease of that organ, by several propositions:—

1. That acute so-called inflammation and disease of the stomach may be entirely free from pain, if the mucous membrane only be affected. Reference was made to the gastro-enteritis of children, and to the symptoms of irritant poisoning. Several instances were adduced, in one of which a large dose of oxalic acid was taken; and except pain in the mouth and throat, there was no suffering, but the patient completely recovered. In a second, a case of poisoning by sulphuric acid, the patient lived eleven days; but except that arising from the action of the acid on the mouth and throat, there was no evidence of suffering, or of pain, tenderness, &c., at the stomach. The whole of the mucous membrane was destroyed, but the deeper structures were uninjured. In a third case, one of poisoning by a solution of chloride of zinc—Burnett's disinfecting fluid—no pain whatever was suffered for three months; but eight days before death pain came on in the left side. Ulceration of the mucous membrane was found near both orifices; near the œsophagus was an opening into an abscess between the spleen and diaphragm; and near the pylorus extravasation was prevented by adherent omentum. It was believed by the author that this abscess had only dated from the commencement of the pain eight days before death, when probably the deeper structures had become involved.

2. That organic disease of the mucous membrane alone—as, for instance, cancer—may be comparatively free from pain. Reference was made to the detection of cancerous secondary tubercles without previous symptoms; and a specimen was shown of a large villous growth from the mucous membrane of the stomach, of which no idea had been entertained, the orifices being free, vomiting absent, and no pain being present for many weeks before death. The patient died from advanced cirrhosis, and at the commencement of her illness had complained of burning pain at the stomach.

3. That disease extending to the muscular or peritoneal coats, produces generally severe pain, as in ordinary ulceration or cancer. Two instances were given in which the intensity of the pain was the most prominent symptom, and in which, after death, the author had detected branches of the pneumogastric nerve involved in the dense fibrous edges of chronic ulcers.

4. That over-distension of the stomach produces severe pain.

5. So also disease of the peritoneum covering the viscus.

6. The statement of Dr. Osborne, that the precise seat of gastric ulcer might be shown by the position of greatest ease, was briefly alluded to; but the author did not give a positive opinion on the subject. In the case mentioned where the pneumogastric was involved, and the ulcer at the posterior aspect, the patient was most comfortable when leaning forward and towards the left side, so far confirming Dr. Osborne's opinion.

7. That in disease of the lesser curvature near the pylorus, pain is sometimes experienced by the patient as soon as the food enters the stomach, and, in some cases, conveys the idea of disease at the œsophageal orifice. An instance was mentioned where, for many months, the affection was believed, by an eminent physician, to be at the end of the œsophagus, whereas the lesser curvature near the pylorus only was affected, and the mucous membrane near the œsophagus was perfectly free.

8. That many conditions of functional disease are entirely free from pain.

9. That the pain in many so-called functional diseases is often exceedingly severe, but is often produced by a mal-condition of the nerves or nerve centres, and arises from the intimate connection of the spinal and sympathetic nerve. Reference was made to the severe dyspepsia occurring in states of exhaustion, extreme nervous prostration, loss of blood, over-anxiety, and uterine disease.

10. That the effect of diseased condition of the pneumogastric nerve at its centre or peripheral branches, in connection with stomach disease, is one of great interest, and it is probable that pain is sometimes the result. The irritability of the stomach in cerebral disease, in disease of the supra-renal capsules, the dyspepsia in phthisis, &c., were alluded to, and several drawings of the nerve supply of the stomach shown.

11. That in many forms of functional disease of the stomach, accompanied with severe pain after food, *it was probable* that extreme irritability of the pyloric orifice existed.

12. That in functional, as in organic disease, pain often arises from distension of the stomach, consequent on chemical decomposition of the alimentary mass.

13. That absence of pain is sometimes found in consequence of the destruction of the pneumogastric nerves. An instance was given, where the whole of the lower part of the œsophagus was destroyed, the pneumogastric nerve exposed, and many branches truncated; fluids had passed into the posterior mediastinum, had burrowed beneath the diaphragm, and made an abnormal opening into the stomach. The patient had travelled, a few days before death, to Guy's Hospital from the North Foreland, and scarcely any pain was complained of, notwithstanding this extensive mischief.

14. That pain at the scrobiculus cordis, simulating disease of the stomach, often arises from spinal disease, the pain being referred to the extremity of the irritated nerve.

15. That severe pain at the scrobiculus cordis, also simulating disease of the stomach, is referred by some, and probably correctly so, to over-distension of the cavities on the right side of the heart, as we find in mitral valve disease, chronic bronchitis, &c. In these instances, the whole of the chylopoietic viscera and the branches of the vena porta are much congested, and the functions of several viscera imperfectly performed.

16. That cancerous disease of the stomach, with enlarged glands pressing upon the aorta, may be simulated by aneurism. In the latter disease, pain is sometimes very intense; and a case was referred to by the author, in which the patient died from the intense suffering, the false aneurismal sac not having given way; and, in dissecting the parts, the large branches of the sympathetic were found by him stretched out upon the sac. No other cause of death was found, after careful examination.

The author stated that these propositions were submitted to the society not with the idea that each was established, but as guides for further thought and observation.—*Lancet*, Nov. 19, 1859.

18. *On some of the Accidents produced by the Development of the Wisdom-Teeth.* By M. Robert.—The four last molars, vulgarly termed "wisdom-teeth," frequently cause, by reason of their slow development, various accidents which the treatises on surgery pass by almost without notice. An interesting and useful monograph might be written upon the subject.

The teeth are short and broad, and have short and straight fangs. They seem to remain behind the others, both in regard to their dimensions and the period of their apparition; so that at first sight, these teeth, which seem to have, so to say, undergone a commencing atrophy, might be expected to make their appearance without inducing irritation. But there are various circumstances which may render this difficult or even dangerous. Thus, it often happens that at the period of the evolution of the large molar it is found to have undergone deviation. As it is always cut at a late period, the other teeth, situated in front, compress its follicle, and distort and thrust back its alveolus. The tooth is thus often found deviating in front or behind, within or outwards. So great is the deviation that the alveolus is occasionally found hollowed out in the ramus of the jaw, the tooth having undergone such a reversion that its crown abuts directly against that of the second great molar. It is easy to see that the eccentric movement executed by the wisdom-tooth at the moment of its evolution, may become, as regards the neighbouring teeth, the jaw, mouth, gums, or even itself, the source of various more or less serious accidents, such as osteitis, periostitis, necrosis, inflammation of the soft parts, etc. This will be easily understood, if we consider the enormous power exerted by the tooth, the effects of which cannot be better compared than to those exerted by water, which, infiltrated into the crevices of rocks, violently separates their fragments, on expanding during congelation.

To these accidents of an inflammatory nature, must be added another, which is hardly ever absent, and which often exists alone during a variable period, viz., the pains that sometimes last for months or years, and depend solely upon the development of the wisdom-tooth. This persistent pain is a nearly certain sign of chronic inflammation caused by obstructed evolution; but sometimes this phlegmasia, under the influence of some slight appreciable cause, or sometimes without any appreciable cause at all, may become transformed into acute inflammation, giving rise to various serious affections of the hard or soft parts, as necrosis, periostitis, abscess, etc. Abscess may form either at the exterior of the jaw, or towards the mouth, whence the pus detaching the muscles and aponeuroses, may descend to the neck. In a case of Bérard's, the pus of an abscess which had formed in the mouth, descended to the subclavicular region, constituting a true migratory abscess, from the effects of which the patient succumbed. Sometimes it is on the side of the throat that the inflammatory symptoms are manifested, and then the enlarged tonsils may have to be excised. In other cases it is towards the velum or the uvula that the inflammation is directed. Thus, an instance occurred to the author in which this was the case, and the hypertrophied uvula, hanging upon the base of the tongue, gave rise to anorexia and vomiting, and other symptoms, which during several months gave rise to the belief in an affection of the stomach. They arose, however, from the inflammation excited by evolution of a wisdom-tooth; and when analogous symptoms are present, at an age in which such evolution takes place, its possibility should always be borne in mind.

These observations were suggested to M. Robert by the presence in his wards of three persons in whom the development of the wisdom-tooth had given rise to various accidents. One of these was a delicate youth of 22, who had suffered from severe pains in the mouth during several weeks. In the furrow between the cheek and the anterior border of the ascending ramus was a vast and very painful ulcerated surface, covered with a whitish deposit; and, lower down, the gum behind the third molar was likewise painful and swollen. The ulceration had been caused and kept up by the swelling of the gum, which itself depended upon the development of the wisdom-tooth. It was proposed to modify the condition of the ulcer by means of fuming nitric acid, and to freely excise the gum over the tooth. In the second case, a woman, aged 22 or 23, had suffered severe pains at the angle of the jaw during two years, and had every now and then

inflammation and abscess in the vicinity. The gum had been incised with temporary relief, and now, after a severe attack of inflammation had been subdued, the tooth was found to be partially through, the gum which covered the remainder being ulcerated. This was excised, and the symptoms all subsided. In the third case, a robust man, aged 32, had suffered from his mouth, at the point corresponding to the last molar, during two years. Abscess, followed by an obstinate fistulous opening, occurred: but as the fistula opened near the chin, and all the teeth were sound, even an experienced dentist had not suspected the true cause. Another dentist, recognizing the nature of the case, drew the second molar, in order to give room for this new one, but without avail. The patient then came to M. Robert, who at once recognized that he had a dental fistula. These fistulæ have, in fact, their peculiarities, being slightly depressed, with the skin around them plaited or wrinkled, and when lying on the lower jaw frequently adhering to the bone. The fistula was traced back with a probe to the tooth, and the latter having been extracted, recovery soon took place.—*Med. Times and Gaz.*, Nov. 12, from *Gaz. des Hôp.*, No. 8.

19. *Hereditariness of the Epilepsy induced in the Guinea-Pig.*—One of the most interesting facts among those discovered by M. Brown-Séquard, is the production of epilepsy in the mammalia, and especially the guinea-pig, as a consequence of certain lesions of the spinal cord. He has frequently repeated these experiments, and with the same results. At a recent meeting of the Biological Society he related the results of the observations he had made during several years upon the young ones born of parents thus rendered epileptic. In some of these he has met with a very distinct epileptiform affection, with well characterized paroxysms, but differing somewhat from the epilepsy of the parents. In the latter, not only are there spontaneous paroxysms, but others may be induced at will, by irritating or pinching the skin of the face; but in the pigs which appear to derive their convulsive affection from their parents, paroxysms cannot be induced in this manner; while the form of those occurring spontaneously is not exactly the same. The animal is first seized with trembling, and then falling on its flank it agitates its limbs spasmodically. The young pigs thus affected have proceeded in about an equal number from epileptic mothers and fathers. Sometimes parents thus rendered epileptic by myelo-traumatism produce little ones, none of whom exhibit the affection, or while some do, others do not. M. Brown-Séquard has had under his observation a very large number of guinea-pigs, and while not denying the possibility of the fact, he has never seen a single animal present an analogous convulsive affection, unless it had previously undergone a lesion of the medulla, or was the offspring of a parent who had been rendered epileptic in this manner.—*Med. Times and Gaz.*, Nov. 12, from *Gazette Hebdomadaire.*

20. *Hypodermic Treatment of Disease.*—Mr. CHAS. HUNTER, late house-surgeon to St. George's Hospital, has published (*Med. Times and Gaz.*, Sept. 10) some interesting remarks on this subject with cases and experiments. The cases detailed are fifteen in number, including two in which the local injection of Dr. Wood was first tried. "A brief review of the cases is the more satisfactory for these reasons—1stly, because in all of them the same narcotic, the acetate of morphia, was used; 2dly, because they had all been under other treatment previously without avail, and in most of the cases it was the same preparation, administered either by the stomach, or skin, which had previously failed.

All the cases were affections of the *nervous system;* nine of the brain, two of the brain and spinal cord, and four of particular nerves.

Although a narcotic, and the same narcotic, was used in every case, it was not always with the same object; thus, in some it was to procure sleep, in others, to ease pain or allay spasm, and in others, again, to attempt to palliate or cure some neuralgic affection.

Three of the cases were *neuralgia*, which had all failed to receive benefit under other treatment, although in each case it had been very varied. The two first received great benefit, firstly, from Dr. Wood's plan, and subsequently

from my own, as stated in the *Med. Times and Gaz.*, Oct. 30. 1858; but the ultimate result of the treatment in these two cases has not yet been given.

Case 1. The man with constant tic-douloureux of four years' duration went out cured, the injection of the morphia being at one time used three times a day, so as to keep up a full and continued influence to prevent, as long as possible, a recurrence of the attack; it was then gradually left off, and the man went out free from neuralgia about a week after the cessation of treatment. The injection was always made in one or the other arm, the site being varied each time; no local inflammation ever occurred, nor was there ever any sickness.

Case 2. The girl with neuralgia and disease of the eye went on with the treatment till the pain gradually diminished; she then left the hospital. The treatment in this case, owing to its nature, could only be palliative. The disease has now attacked the brain (having destroyed the eye), and she is now (June) being treated by this plan of treatment in another of the London hospitals, as nothing else seems to ease the pain or give her sleep. No sickness ever followed the injection of narcotics, but frequently did their administration by the stomach.

The third case of neuralgia was cured by a single injection, although the case had resisted many other forms of treatment. Sickness took place in this case, and was considerable.

The cases of delirium tremens were two. Morphia in the first, and opium in the second, had been given in large and repeated doses, with no decided effect. In both, the first injection caused sound sleep of many hours' duration; and both went out cured in a few days. There was no sickness in either case.

The cases of *mania* were two. They both showed that sleep could rapidly be obtained by the injection of morphia, which effect had not been obtained by doses as large, and larger, given by the stomach. There was no sickness in either case.

In the first case of *puerperal mania* the injection was used only once; sleep followed in eight minutes. The patient subsequently recovered by the internal administration of large doses of Battley's sedative. In the other case, several injections were employed, sound sleep following each time. There was no sickness in either case.

In both the cases of *wakefulness*, sleep was rapidly induced; and in one patient excitement, almost amounting to delirium, was quelled by a single injection.

In the case of chorea the effect of the injection was always very rapid, sleep resulting in about four minutes, if the quantity injected was large, and if small, the violence of the movements was diminished or arrested. No sickness was ever produced.

In the case of tetanus sleep resulted at once, although the spasms remained unaffected. In a second case of tetanus, in which this treatment has been used, two hours' sound sleep followed the injection, and afterwards the patient dozed for several hours; laudanum had been previously given every hour without effect. The spasms were never violent in this case, and were observed to cease during sleep.

Both cases of sciatica went out free from all pain—the one that was cured was a little sick after the first injection; in the other case, the pain returned in a milder form.

Such was the result of the treatment in the cases already given; the result was different in the various cases, even as the object was different with which the injection was employed. Sleep was the result in most cases, but not in all; spasm was quieted in some, and pain was relieved or cured in others by a single or by more injections.

With regard to the sleep occasioned by the injection, it must not be looked on as a necessary effect of the treatment—it may follow at once, after a time, or not at all, according to circumstances. Thus it may follow—*At once*, if the quantity injected is large, and the object is less to ease pain than to procure sleep. *After a time*, if the quantity is large (say one grain of morphia), and much pain exists; in which case the pain is generally quieted directly, and sleep follows in from fifteen to thirty minutes. *Not at all*, when the quantity injected is small, and there is much pain, spasm, or cerebral excitement going on, in

which cases, as the quantity is small, the whole effect of the narcotic is expended either in subduing the pain and spasm, or allaying the excitement.

The Occasional Sickness.—In the fifteen cases, sickness occurred *in only two*, in one it was distressing, in the other but very slight.

In *another series of fifteen* cases, of which I have notes, sickness occurred in *four*, in one it was considerable, and in the others to no extent.

Consequently in *thirty* cases only *two* patients, both of them women of very nervous temperament, and both suffering from tic-douloureux, had considerable sickness; in the other *four*, all men, with sciatica, the sickness was trifling; in fact, the patients themselves thought nothing of it, nor did they think it due to the injection. This cannot be called a large proportion when it is recollected how often morphia causes sickness when given by the stomach; I have constantly seen it in quarter of a grain, and laudanum in equivalent doses, cause sickness, so administered.

The *time* when the sickness comes on varies; in the two cases in which it was severe, giddiness and nausea were felt almost immediately, then faintness, till in about five minutes sickness took place; in both it continued on and off for several hours, with intervals of sleep. In the four other patients the sickness did not come on for many hours, in fact, only as a kind of ultimate effect of the morphia after a good sleep."

This plan of treatment, Mr. H. admits, is no specific, and therefore *a due discrimination of cases ought to be made.* "The same caution is given by Dr. Wood in his paper on the treatment of neuralgia by *local* narcotic injections: 'Another caution I would offer is, that you choose the proper patient for the use of the remedy.'[1] When to employ the injection, and when not, must depend in the general way on the particular circumstances of each individual case, such as the nature of the disease, its urgency, the object in view, etc.

There are some cases in which I think the hypodermic injection may almost be employed *as a rule*, and be put in force *before time is lost* by the adoption of other measures. I mean those cases of high cerebral excitement, of delirium tremens, and of mania, in which the speedy administration of a narcotic is indicated. In this class of cases more than any other the value of the injection is seen. I have already detailed *seven* cases of this nature, and could give many more, but there hardly seems the necessity. In these cases, to procure sleep and allay excitement is the object, and that as soon as possible; the stomach is often irritable, or in such a state that it will not absorb medicines; the patients often refuse to swallow (as in Mr. Cutler's case); everything, in fact, points to the necessity of some more sure, speedy, and active mode of treatment than the more ordinary one of stomachic administration.

There are cases of sudden, violent, acute pain, in which the injection might also be tried as a *primary measure;* for instance, during the passage of a renal calculus, in such a case the pain is at times almost insupportable, and as the stomach gets quickly irritable, sickness often taking place, another reason is furnished for the trial of this plan.

Then there are cases in which the injection ought not to be tried at first, but after such general treatment as is clearly indicated has been first tried—such as purging, or the internal administration of alteratives or tonics. Tic-douloureux, sciatica, and many other neuralgic affections are of this class. In all such cases, due discrimination being made, it is astonishing what benefit follows the injection in most cases, and how quickly in many, a cure is effected by it when other treatment has altogether failed. It is often, too, in those cases the origin of which is most obscure, that the injection seems to answer best.

Rheumatism is a disease in which occasionally the use of the injection into

[1] My object in alluding to this passage (*British Medical Journal*, August, 1858) is this—that as I do not find localization to the painful part necessary, my plan is available in many *diseases* in which that of Dr. Wood is not, as mania, delirium tremens, etc. It is also applicable in some cases of pain and of neuralgia in which Dr. Wood's cannot be—viz., those in which the nerve cannot be reached for injection. There is, therefore, a greater need of caution to discriminate proper cases where the range is *wider*, than where it is more limited.

the cellular tissue will be found highly serviceable. Two cases have been treated by it at St. George's Hospital."

The choice of a narcotic, Mr. H. says, "must depend entirely on circumstances, such as the sex, the peculiarities of the patient, the disease, and the object in view. It is not my intention here to go into these points, as they require *almost* equal consideration before the administration of a narcotic by any method, but rather to indicate in this place the *most eligible preparations for injection.*

Tinctures may, and can be, used with good effect. Thus Mr. Burns used equal parts of the tinctures of opium and hyoscyamus.[1] I have employed both these tinctures separately, and have found this objection to them, that they cause a little hard lump beneath the skin which *may* last a considerable time, and gradually disappears if left quiet, and alone. There is not this objection if the tincture be evaporated to one-half or one-third, and be used while it is fresh.

Dr. Wood has used a solution of morphia in sherry wine as 'it would not irritate and smart so much as alcohol.' He also says that 'nepenthe' produces less sickness than opium, and is therefore preferable as an injection.[2] Professor Simpson has used a *solution* of the bimeconate of morphia in coccodynia.[3]

For my own part, I *prefer solutions to tinctures;* they are rapidly absorbed, they produce no irritation if properly made, and they have this advantage, viz., of exactness, so that no mistake need be made about the strength of the preparations, or the quantity injected.

A solution of the acetate of morphia is the preparation I have used more than any other, prepared with acetic acid, but so freed from excess of that agent that it causes not the least irritation.[4] Mr. Williams, of Liverpool, who has tried the narcotic injection in delirium tremens, has proposed to me the employment of the sulphate of morphia on account of its ready solubility.

A solution of the sulphate of atropine is a good preparation for injection, and not liable to irritate. I have several times employed it, and produced sound *sleep* with doses varying from the $\frac{1}{5}$ to the $\frac{1}{16}$ of a grain. The injection of this salt has been employed with a different object by Mr. Benjamin Bell, viz., to counteract the effects of opium poisoning as first suggested by Dr. Thomas Anderson."[5]

Chloroform, Mr. H. states, may be, with safety, injected into the cellular tissue in *urgent* cases; it rapidly produces cessation of spasm, and causes sleep.

"In numerous experiments which I have made with this narcotic on animals, I find that the effect on rabbits, when injected in small quantities, is somewhat analogous to that of opium in small doses, but in larger is more productive of anæsthesia than of coma."

Having a patient suffering frightfully, Mr. H. states, "from neuralgia of years' duration, on whom all ordinary medicine was lost, I was tempted to endeavour to give him sleep and relief from spasm for a time by the injection of chloroform. Although accustomed for years to inhale, and even swallow, enormous quantities of that agent, and to take other narcotics in similarly large doses, the first injection of ♏xxx of chloroform, slowly introduced, caused almost instant quiet of spasm, and sleep in fourteen minutes. The injection of chloroform was several times repeated at intervals, at the urgent request of the patient, who each time obtained considerable relief from it. I left it off, however, on account of the local symptoms, which took place at some, but *not all*, of the places injected; and because of these local effects, I think the injection of chloroform is *not to be recommended.* These local effects were—firstly, pain at the moment of injection; secondly, in a few minutes the skin of the part injected became of a bright red colour, elevated, and tender to the touch; and giving, thirdly, for a short time, the feeling of crepitus. At the end of twenty-four hours all these symptoms disappeared, the swelling going away, and the redness being replaced by

[1] Medical Times and Gazette, October 16, 1858.
[2] British Medical Journal, August, 1858.
[3] Medical Times and Gazette, July, 1859.
[4] Ibid., March 26, 1859.
[5] Edinburgh Medical Journal, July, 1858.

a brown stain like a macula. From ten to fifteen days after, nothing was to be seen or felt, although a sensation of tenderness or pain was still experienced in the part by the patient, *but no abscess resulted at any point.*"

The following are Mr. H.'s practical conclusions:—

"1. That certain medicines may be introduced into the cellular tissue beneath the skin with safety and with advantage.

2. That medicines so introduced have a *general* as well as a local effect.

3. That the general effect of medicine so introduced is exceedingly rapid.

4. That this mode of administration is *more certain in its action* than stomachic doses are, for the *exact* amount introduced is known, and the whole of it takes effect, which *may* or *may not* be the case with stomachic doses.

5. Medicines are *more purely received* into the system by this method than when given by the stomach, in which organ they may become contaminated or decomposed.

6. A given amount of a medicine employed hypodermically has a greater effect than the *same* amount administered by the stomach; *it also acts more quickly.*

7. A given amount of a medicine employed hypodermically has a greater and more rapid effect than when employed *endermically.*

8. That the medicines for which this mode of introduction is especially applicable are the various *narcotics* and *sedatives.*

9. That the *diseases* for which this plan of treatment is especially indicated are for the most part *affections of the nervous system*:—

1stly. Where the immediate and decided effect of a narcotic is required.

2dly. Where narcotics administered by the usual methods fail to do good, and yet are indicated.

3dly. Where the effect of a narcotic is required, and the patient *refuses to swallow.*

4thly. Where, from irritability of the stomach, or other cause (such as idiosyncrasy, etc.), the patient cannot take the medicine by the stomach (Case 2).

10. That to produce a general effect, it does not signify whether the remedy be injected into the cellular tissue of the body or of an extremity.

11. That to relieve or cure a local neuralgic affection there is no necessity to localize the injection.

12. That whether the object be to treat a local or general affection, it seems advisable each time to change the site for injection, should it be more than once required."

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21. *Chorea Treated by Sulphate of Zinc.*—Dr. WM. H. STONE, Medical Registrar of St. Thomas's Hospital, states (*Med. Times and Gaz.*, Sept. 17, 1859) that during the year 1858, there were admitted into St. Thomas's Hospital 54 cases of chorea. Out of this number, 50 were put under the influence of three principal remedies. In 16 of these, the treatment mainly comprised the exhibition of sulphate of zinc; in 20, of arsenite of potash; in 14, of ferruginous preparations.

Of the 16 cases treated with sulphate of zinc, five were males and eleven females. The ages of all were intermediate between 8 and 16 years. Previous attacks had occurred in two cases only. Duration of previous illness had been one week in two cases; two weeks in three; three weeks in three; a month in two; two months in two; three months in one; four months in two, and a year in one case. The attacks were without known cause in four instances, from fright in four, from a fall in one, and followed previous illness in five cases. This illness was in one instance of febrile character, probably scarlatina; in another measles, and in three others acute rheumatism; of these latter cases one also suffered from cardiac disease, ultimately terminating in death. Two other cases, though not absolutely complicated with illness, showed evidence of being connected with commencing menstruation in a feeble habit. One case was attacked with varicella soon after admission.

As a general statement few of the above cases were characterized by great severity, and of these one, Case 12, which warrants such a description, was more remarkable for its obstinacy than for the acuteness of the symptoms. Accordingly, the graver disturbances of function are comparatively infrequent. Inter-

ference with articulate speech was only observed in six of the cases; dysphagia, to an extent beyond what would naturally follow from spasmodic contortions of the face and hands, only in one case. The rapid and spasmodic protrusion of the tongue, to which much attention has been drawn as a symptom of chorea, was observed in five cases. They formed the bulk of those which have already been mentioned for imperfection of speech. The one remaining case had recovered considerably from this latter symptom before admission, and it is probable that, at an earlier period, spasm of the tongue might also have been substantiated. Distinct spinal tenderness was only found in four cases.

Sulphate of zinc was alone exhibited in eight cases. Of these, five were cured. Three were improved, though not cured. It was in all cases administered in increasing doses, beginning with one or two grains. Six grains was the highest dose given in the cases before us, though in others four times this amount has been exhibited with advantage, and without vomiting. The shortest effective administration was of twenty-four days' duration among the cured cases; the longest of fifty-six days. The average period of continuing the medicine was twenty-nine days for all the cases; two of which were removed before its full influence had been produced, and one was interrupted by an attack of varicella. In four cases the sulphate of zinc was followed by ferruginous preparations. One of these, Case 5, complicated by rheumatism and cardiac disease, must be regarded as an exception. In the other three, Cases 1, 12, and 15, the remedy must be considered to have failed, or only to have succeeded partially; they were all ultimately cured under the administration of iron.

In four cases, the zinc was followed by a course of liq. arsenicalis. In three of these, Cases 3, 7, and 11, this effected a cure; but in the other, Case 12, it was equally ineffectual with the zinc, and recovery took place under the iron treatment. In Case 16, on the other hand, the liq. arsenicalis was exhibited for eleven days in doses of ♏v ter die, without much effect, and recovery took place after the administration of sulphate of zinc for nineteen days in doses of gr. iij ter die. In two cases, 5 and 13, opiates formed part of the treatment. The former of these required their use principally from rheumatic and cardiac complication; in the latter they seem to have had a beneficial effect; inasmuch as recovery took place under their use combined with iron, though the disease had previously resisted both remedies when exhibited singly.

Besides the principal remedy, thirteen cases had the cold or tepid shower-bath during the treatment. Of the three not so treated, one was confined to bed with subacute rheumatism and cardiac symptoms; the second had only recently recovered from an attack of measles; the third had varicella soon after admission. It is not possible to give any separate estimate of the value of this application in bringing about the cure, though it cannot but be ranked very high. It may be omitted as an element of comparison, from its employment in all cases which did not exhibit a distinct counter-indication.

The general statistics are as follows: Of 16 cases treated with sulphate of zinc, 13 went out cured; 3 relieved; but 2 of the latter were in a fair way of recovery, and may probably be set to the credit of the medicament. On the other hand, three of those ultimately cured, owed their improvement partly to ferruginous preparations; and in one case the zinc had no effect whatever. It may, then, be stated generally, that advantage was derived from the zinc in 14 out of 16 cases. The longest stay in the hospital among these cases was 123 days; the shortest 14; the average stay 44.6 days.

Fourteen cases were treated during the same period with preparations of iron; all were cured. The longest stay in hospital was 161 days; the shortest 6 days; average stay 44.2 days.

Twenty cases were treated with liq. potassæ arsenitis: 18 cured; 1 relieved; 1 died. The longest stay in hospital was 55 days; the shortest 6; average stay 26.3 days. Average stay in hospital of the 50 cases submitted to three principal remedies, 37.2 days; average stay of all the 54 cases, 35.4 days.

The results of this analysis are somewhat remarkable, as failing to confirm the usual estimate of the value of sulphate of zinc in this disorder. The iron seems to act more certainly, and the arsenic both more certainly and more rapidly than the zinc. The average duration of treatment both with iron and

zinc, 44.2 days and 44.6 days respectively, is very similar, and both are above the general average of the whole number of cases, namely, 35.4 days; whereas, the average stay of the arsenic cases falls as low as 26.3 days. This difference is the more remarkable, as the character of the cases submitted to the arsenical treatment rather exceeded in severity that of the others; and, indeed, the only death recorded belonged to this division.

It remains a question whether the discrepancy between these results and those of some previous well-conducted observations is due to mere accident, or to some real difference in type between cases originating at different times and under dissimilar circumstances.

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22. *Iodide of Potassium in Diseases of the Brain in Children.*—Upwards of twenty years since the iodide of potassium was recommended by Roeser and others as a remedy of special power in hydrocephalus. Dr. JOHN COLDSTREAM states (*Ed. Med. Journ.*, Dec., 1859) that his own experience has led him, for a considerable time past, to its employment, almost exclusively, in the treatment of those numerous ailments of children, which we cannot but regard as indicative of a tendency to hydrocephalus. In all cases in which, from the course of symptoms, I have reason to believe that the central organs of the nervous system, or their envelops, are in any degree affected with strumous inflammation (tubercular cerebritis, or meningitis) or its consequences, after moderate purging, and perhaps application of leeches to the head, I am in the habit of prescribing the iodide, in doses of from half a grain to three grains, every three or four hours, generally dissolved in some carminative water, and continuing it in doses, varied according to the symptoms, for many days, or even until convalescence is fully established; and I am quite satisfied that, under this treatment, with the occasional addition of blisters to the shaven scalp, I have seen far more prompt and decided effect produced upon the disease than I used to see under the old treatment.

When opportunities have been afforded of commencing the use of the iodide early, it has appeared in several cases to arrest the progress of the disease *rapidly*, so that the formidable effects of effusion, indicated by squinting and convulsions, have not supervened. In less favourable circumstances, in cases where considerable prostration had succeeded to great febrile action, and in which starting and squinting had become prominent symptoms, I have seen, in not a few instances, the free use of iodide of potassium followed by amendment and complete recovery. In such cases, and in others still further advanced, I have generally given larger doses, even to the extent of four grains, several times a day, to children of from four to eight years of age.

The medicine is very seldom refused by the patient, and I cannot say that I have ever seen it either increase the nausea that so frequently exists in the earlier stages of the disease, or produce any other untoward effect; especially have I never seen it induce salivation, which the drug sometimes seems to cause when given for other ailments.

It seems generally to act upon the kidneys; yet I cannot say that the amount of relief to the head-symptoms bears any very obvious relation to the quantity of urine excreted.[1]

Although I have no doubt that the iodide is more especially useful in cases where there exists more or less of the scrofulous diathesis, I have often used it with satisfaction in patients apparently free from all such taint; even in cases

[1] In a paper, "On the Diuretic Action of Iodide of Potassium" (*Arch. of Med.*, No. 3, London, 1858), Dr. Handfield Jones remarks, that "there are certainly remedies which exert very positive curative influence, admitting of no doubt or question, yet which afford no clue in their general mode of action to explain their special effects. Such, it appears to me, is iodide of potassium." Dr. Jones' observations lead him to conclude that, under the use of iodide of potassium, the quantities of water, of phosphoric and sulphuric acids, and of chlorine in the urine, are very much increased; but the knowledge of this effect of the administration does not enable us satisfactorily to explain its *modus operandi*, either in the cure of secondary syphilis or in that of tubercular meningitis.

where the ailment seemed to have followed injury from external violence, as so often happens in young children. I am not prepared, however, to assert that the iodide is more useful than calomel in *all* cases of inflammation of the brain and its appendages. When we have to treat robust and full-blooded children, in whom there is good reason to believe that the threatened disease of the nervous system stands more or less directly connected with the preceding disorder of the digestive organs, I have no doubt of the superior efficacy of the mercurial treatment, combined with antimonials and salines; but when, after having duly administered these, symptoms of cerebral disorder continue, I would have recourse to the use of the iodide.

In cases of convulsions from teething, which, amongst ill-fed children, living in badly-aired localities, are not unfrequently followed by hydrocephalus, I have used the medicine with much satisfaction.

I have occasionally employed the proto-ioduret of mercury, as advised by Evanson and Maunsell, but not with more obvious benefit than I have been accustomed to see resulting from the use of the iodide of potassium. During convalescence, I generally prescribe the iodide of iron; sometimes a vegetable tonic, combined with the iodide of potassium.

In several cases of recovery from severe attacks of meningitis, it has occurred to me to find the mental powers of the little patients considerably impaired. This result has occasionally been protracted for many years, and seems likely to prove permanent; but, generally, it has gradually become less apparent, and ultimately passed off entirely.

In thus endeavouring to recall attention to what I believe to be a truly valuable agent in the treatment of a class of formidable diseases, I would not overlook the fact, that all past experience tends to assure us that a great majority of cases of disease of the brain in early life prove fatal under all kinds of treatment. In advanced stages of the tubercular forms of these diseases, we may not yet venture to hope for any great advantage in the use of the iodide of potassium. But I am disposed to agree with Drs. Copland,[1] Willshire, and West, in believing that they may be cut short, if subjected to treatment in an early stage, more frequently than is generally imagined. My own experience leads me to regard the iodide as more likely than any other drug to promote this desired end; and my confidence in it, as *the* remedy best adapted to all stages of tubercular diseases of the head, is so strong, that whatever else might be done, or left undone, I would persevere in administering it, even in circumstances the most desperate. In almost all diseases of children, it appears to me right to continue treatment, even to the last. I am very fully satisfied that the use of the iodide never produces any bad effects, however frequently it may fail to do good.

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23. *Chlorate of Potass in Mercurial Stomatitis, and in other Diseases of the Mouth.* By M. J. V. Laborde.—We give the conclusions arrived at in this interesting and important memoir, which obtained the Corvisart prize for 1857.

The chlorate of potass exercises a *distinctly curative* action. In no case tested was this action found deficient.

It possesses, besides, a preservative or prophylactic action, which permits of the administration, during one or two months, of the protiodide of mercury in doses of from fifteen to twenty centigrammes a day without the least injury to the cavity of the mouth; and the proof of such immunity being due to the chlorate of potass, is found in the fact of mercurial stomatitis being at once developed upon leaving off its use.

The time required for treatment of confirmed stomatitis varies with the intensity of the case. In ordinary instances, it never exceeds four days; in

[1] "If recognized early, a large proportion of cases will recover; even in the most advanced periods the patient should not be despaired of. I have repeatedly seen recoveries take place, although strabismus, paralysis, convulsions, blindness, unconscious evacuations, and other unfavourable circumstances, had existed some time." (Copland, *Dict. of Pract. Med.*, i. p. 668.) "The prejudice which attributes the character of incurability to tubercular meningitis only serves the purpose of shackling the progress of medical art." (Hahn, *De la Meningite Tuberculeuse.*)

some extraordinarily severe cases, eleven days have been required. In almost every case, the first indications of amendment have shown themselves on the second or third day; and, according to what has been observed, the order in which these manifest themselves is: 1. In diminution and entire relief from pain; 2, in diminution of the salivation, and of sub-maxillary or parotidean swelling, when this is present; and 3, in the almost simultaneous disappearance of swelling of the gums, and a return to their natural colour with the disappearance of ulceration where that has not existed previously to the stomatitis, in which case, of course, the chlorate of potass is ineffectual.

The increase of the dose does not seem to add to the rapidity of cure, except in very bad cases. In those of medium severity, the dose of three or four grammes is sufficient; and its administration in the form of a julep is preferable to all other modes.

When employed as a gargle, it seems very effectual in cases where the symptoms are exclusively local, such as in mere swelling of the tissue of the gums, their discoloration, ulceration, etc.

It action appears to be entirely localized to the mouth, as is evinced by its being found in the saliva, as well as its effects being exhibited in the mouth when administered internally; and it appears to act as a sort of specific against the effect of mercurial inflammation in this quarter. At the same time it does not interfere with the therapeutic action of this remedy.

Of nine cases of chronic gingivitis, eight of which were accompanied with alveolar suppuration, six cures were effected under its use; two cases were unsuccessfully treated in the same manner; and the one case, without suppuration, was cured only after a prolonged course of treatment.

In only one of these successfully treated cases of gingivitis was the remedy given internally. The cure was complete in twenty days, and the first symptoms of amelioration were manifested about the seventh day. In five other cases, in which the remedy was given internally, no effect was produced by it; but on being used as a gargle, its good effects were speedily apparent—four or five grammes at a time, used in this way, leading to recoveries in from three to eleven days.

The good effects of such treatment almost always begin to manifest themselves in these cases about the second day, and consist at first in a well-marked improvement of colour in the gums, the diminution or cessation of purulent discharges, and the relief of pain. Even in the most obstinate cases, the swelling, discoloration, and pain soon subside—the suppuration alone being difficult to deal with. And in every case the dental tissues themselves rapidly recover their normal whiteness, however much this may have been altered.

In conclusion, it may be remarked that the chlorate of soda very closely approximates in its qualities the chlorate of potass; its solubility is greater, and its taste less disagreeable. Both are, therefore, applicable and efficient remedies in acute ulcerous stomatitis.—*Edinb. Med. Journ.*, Aug., 1859, from *Gazette Médicale*, May, 1859.

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24. *Paralysis Agitans Removed by the Continuous Galvanic Current.*—Dr. J. R. REYNOLDS relates (*Lancet*, Dec. 3, 1859) the following case of this:—

W. F., male, aged 57; married at the age of twenty, and the father of twelve children; height, 5 ft. 10¼ in.; weight, under 11 st. No anatomical deformity; no hereditary predisposition to disease; has had good health; has lived well and temperately. His occupation is that of a carpenter; he has resided in a healthy locality, and has never, until the commencement of his present illness, suffered from anything of a similar kind.

For the last five years he has had anxiety with regard to his children, and distress at parting from them, but he cannot definitely refer his malady to this cause. During the last two years he has noticed occasional tremor of the right arm and leg, the latter being affected less frequently and less severely than the former. The tremor has occurred if he (1) has been "put out of anything;" (2) has attempted to lift anything very heavy; (3) has "taken cold;" (4) has lifted liquid in a cup to the mouth; or (5) has fully extended the arm and forearm, and pressed anything firmly with the palm of the hand. But under all these

circumstances the tremor has ceased when the "exciting cause" has been removed, and it has never been so severe as to prevent him from following his occupation, which is one requiring much exertion and accurate direction of movements.

For the last six or eight months he has suffered occasional vertigo—*i. e.* a "feeling as if he should fall, or pitch on his head; and as if the head were tied up in tight bandages." At the same time there has been darting pain through the head.

On September 20th he was at work as usual—was alternately stooping down and lifting over his head—when he suddenly felt vertigo, aching in knee-joints, and general disturbance; and at the same time violent shaking occurred in the right upper extremity. The agitation of the right arm continued throughout the day, but stopped at night. It returned on the following morning, as soon as he moved.

On October 5th he was first seen by myself, and on this day (the fifteenth from its commencement) the agitation was extreme. Nevertheless, it had always ceased during the night; and, on two occasions, for about an hour, and without assignable cause, during the day. He thinks it is arrested at night by pressing the anterior surface of the forearm against the crest of the ilium. With the exceptions above mentioned, the movements of the arm have been much the same as now seen; being occasionally aggravated, but not much, by emotional disturbances, or by the attempt at voluntary movement of the extremity.

The whole of the right upper limb is involved—*i. e.* the hand moves on the forearm, the forearm on the arm, the arm on the shoulder; but the most constant and most extensive movement is that at the elbow-joint; the least constant and least extensive is that at the shoulder. Almost every direction of movement possible in the upper extremity is performed; from 22 to 24 double movements occur in five seconds, and the range of movement at the hand, when, for example, the jerking is principally that of flexion and extension of the forearm, varies from nine to ten inches. The movement, therefore, amounts to about eight feet per second.

To the patient himself the right arm feels hotter than the left, and a difference of temperature is very obvious to the hand of the observer. Temperature over left biceps, 87° Fahr.; over right, 91°.

The involuntary movement of the arm can be arrested by his lying on the sofa, and pressing the forearm against the ilium; but any attempt to move the limb voluntarily at once reproduces the shaking, although he remains in the recumbent posture. The movement is, moreover, instantly arrested by my firmly grasping either the forearm in any part of its upper two-thirds, or the arm in its lower third. This is not a mere mechanical arrest of the movements, for it cannot be effected by holding the wrist; and the jerking recommences if, while the extremity is grasped in the manner described, the patient makes any attempt at a voluntary movement. The pressure is not painful, nor is it so directed as to arrest the circulation.

The mental condition of the patient, and his general health, appear unaffected.

Sensibility is unchanged in the right upper extremity; there is no deviation of the tongue, nor distortion of the features. He can walk well, and without dragging either leg; there is only occasionally slight tremor of the right leg.

A continuous galvanic current (direct) was applied to the arm and forearm, the movements of the latter being at the time arrested by pressure. At the end of five minutes he could execute voluntary movements without the least tremor, and emotional excitement failed to reproduce the jerking. The temperature of the two arms, examined after the current had been passing for half an hour, was equal. The involuntary movements did not return until three hours after the current was discontinued; they then reappeared, and continued throughout the evening; stopped at night, but returned on the following morning.

October 6. The current was applied while the arm was in violent movement, but in two minutes it became perfectly still. Application continued for an hour.

7th. Last evening there was no jerking nor tremor for five hours after the

current was discontinued; then it commenced, but stopped spontaneously in about half an hour, and during the remainder of the evening there was nothing more than very trifling tremor. The jerking has returned this morning, but is much less than on the first day of observation. There are but twenty alternations in fifteen seconds, and the range of movement is from three to four inches. The movement, therefore, is only .86 foot per second—less than one-eighth of what it was three days ago.

The current was applied on the 7th, on the 8th, and 10th, and after the 10th —*i. e.* after five applications—the spontaneous jactitation completely ceased. When any weight is held in the hand, and it is lifted towards the mouth, there is tremor; but this is slight, is not more than has occurred for the last two years, and it immediately ceases when the effort is discontinued. The arm and hand are weak; every movement can be executed by them voluntarily, but such movements are feeble.

28*th*. Has written me a letter in a good and legible hand.

The current was applied about every other day, for an hour, until Nov. 10th, and during this time there was steady increase in the power of the limb, and the jactitation did not return. No medicine of any kind was given.

November 12. Quinine and iron were ordered.

15*th*. W. F. is in perfect general health; there is no jactitation, and only the slight tremor already described when the hand, with something in it, is raised towards the mouth.

The current employed in this case was derived from a Pulvermacher's chain battery of 120 links.

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25. *Preparations of Larch Bark in Pulmonary Hemorrhage.*—In our number for July, 1858 (p. 201), we noticed favourable results obtained by Drs. Moore, Kennedy, Carmichael, &c., from the use of the larch bark. Dr. OWEN DALY states (*Med. Times and Gazette*, Nov. 12, 1859) that he has used with equal success this article. He prescribed it first for an out-patient at the Hull General Infirmary, suffering from pulmonary hemorrhage, for whom he had, without the slightest benefit, prescribed all the usual styptics: he resolved to prescribe the tincture of bark. The patient was a female, aged 32, a widow in an advanced stage of consumption. She complained of great difficulty in breathing, of oppression at her chest, and of a constant cough, attended with expectoration of blood. The tincture of larch was ordered in twenty-drop doses every third hour in a little water; the relief was immediate; at the end of a week the hæmoptysis was entirely arrested; and her other symptoms were so much mitigated that she soon ceased to attend at the infirmary.

The styptic properties of the larch bark had, in this case, so far exceeded my expectations that I thought it deserving of a more extended trial, and up to the present time, I have prescribed it in fifteen cases of pulmonary hemorrhage. The cases have not been selected—some have been cases of active and severe hæmoptysis, others of a passive and more chronic character. I have also employed it in one very severe case of epistaxis, and in a case of chronic cystitis, and with the exception of this latter case, in every instance with the most satisfactory results.

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26. *Elements of Prognosis in Phthisis.*—Dr. POLLOCK read a paper on this subject before the Harveian Society (May 19, 1859), which he illustrated by the exhibition of several photographic portraits of patients in whom the disease had been arrested.

The following is an abstract of the paper:—

Dr. Pollock began by observing that the subject of phthisis had fallen into disrepute with medical men, who were accustomed to bestow the largest amount of attention only on what is new and striking. The prevalent idea that the disease is incurable, and that its progress is only a continuous declension towards the fatal result, also retarded new researches—the profession conceiving that it was to be palliated, but never eradicated. This condition of the professional mind was most to be deprecated, as from it would spring no progress, and under its influences even an impartial examination of facts became impossible.

It were inflicting a mental blindness on ourselves to allow this state of things to continue without a protest occasionally from those who have large opportunities of seeing this disease. For some years the author had enjoyed these opportunities at the Consumption Hospital; and he proposed, without reference to theories, to offer a few rules for guidance in forming an opinion on the probabilities regarding the future course of any case of phthisis. The value of correct prognosis was very great, involving the personal interests of the patient and his friends, and the character of the physician. The applause and the highest pecuniary rewards of the public also attend on our proficiency in solving these questions; and the author conceived that he should not exaggerate their importance if he stated that more fortunes had been made by scientific accuracy in prognosis, and more credit lost by mistakes in the same, than by all other incidents of professional life put together. The recent practice of insurance offices to accept diseased lives at an increased premium, also added to the importance of an accurate knowledge of the subject. To the attainment of this careful observation and precise knowledge, must be added individual tact. In speaking of consumption, he must be understood to mean a deposit of tubercle indicated by the known physical signs. The first stage meant a simple deposit; the second, its softening; the third, an excavation. Medical language and that of the public greatly confuse one another: the latter desiring to indicate the degree of danger alone. Patients die in all three stages: a fatal result often occurring with extensive deposit only, which had never softened—the sick person being thus in the first stage of tubercle, but in the last of the disease. Let it at once be stated that the degree of disease in the lung and the condition of the patient cannot be expressed by the same formula. Physical signs can never be the measure of the danger, any more than symptoms alone: both must be studied together. It was first necessary to ascertain the average duration of the disease—all stages and varieties being put together. Here we found great difference of opinions, of which the following is an abstract: Portal says it may last from 1 to 40 years; Louis and Bayle in 314 cases found the mean to be 23 months—of these, more than half (162) terminated in 9 months, and the greatest proportion between the third and ninth month; the average duration was 18 months. Andral, at La Charité, found the average duration 2 years; Sir James Clark, in the upper classes "enjoying advantages," 3 years; Dr. Williams, assuming the average duration to be two years, considers (in the last edition of his work) that it has been doubled, or raised to four years, by the introduction of cod-liver oil as a remedy. Three years would be the medium of these opinions. The author inclined to place the duration at a much higher figure, and was satisfied that if cases were earlier recognized, they would be found to last much longer than now supposed. The method of invasion of the disease, and its progress, were next dwelt on. It was evident from the study of some thousand cases which have been under the author's care, that phthisis proceeds by a succession of attacks, and he doubts if even galloping consumption is ever present without a previous, but perhaps unnoticed attack—an opinion which has been expressed by Louis. The symptoms and physical signs of such an attack of tubercle were then described, the patient presenting with slight dulness of percussion, prolonged expiratory murmurs, roughness of respiration, and slight increase of vocal resonance—the symptoms being fever, more or less severe, of a remittent form, cough, and slight evacuation, with or without an hæmoptysis. From this there is partial recovery, but the patient ever after remains below his normal standard of health. Dr. Pollock has seen perfect recovery occur, though rarely, the physical signs being occasionally quite removed. Now, it is obvious that if the subsidence of the attack were overlooked in prognosis, we might deliver a false opinion as to the danger of our patient. A second invasion of disease invariably occurs at a shorter or longer interval. The circumstances which might be considered favourable as regards duration (or the disease becoming chronic) were then dwelt on. These are the softening being limited in extent; the fresh deposit occurring in the same lung lower down; and a clear respiratory sound persisting at the base. The limitation of disease to one lung, whatever be its extent or stage, is more favourable for prolongation than if the affection be double, and the stage earlier. Two varieties of very chronic phthisis

were then described. One, a limited excavation, of well-defined characters, in one apex, the physical signs showing consolidation of the lung tissue below this, and the base being quite free from deposit; the other consisting in a cessation of all activity on the stage of softening, and a coincident improvement in symptoms. The physical signs evidence a diffused deposit, shown by a crepitation of a dry character, with here and there a bubble, intervals existing in which the respiratory sound is bronchial and dry, and nothing to indicate a cavity in any part. The expectoration is moderate, the cough confined to certain hours of the day, the health tolerable, but on a low par, but there is considerable dyspnœa on exertion. Dr. Pollock has known such cases last as long as fifteen or twenty years in persons who outlived middle age; and some interesting photographs were handed round, of young girls who had been for some years in this condition, who appeared in tolerable flesh, and were in good general health, the menstrual health becoming established, with improvement of all the symptoms. It would thus appear that there are two kinds of tubercle, one prone to rapid softening, the other presenting inert features. Dr. Pollock also cited cases to prove that there occasionally exist cases of strumous deposit in the lungs in young persons, analogous in all respects to that in the cervical glands, and which become slowly absorbed in the same manner. The prognosis in children must therefore be most guarded. The importance of examining *the whole chest* was then dwelt on in forming an opinion as to the probable duration of a case. The localization of the deposit is worth observing. Softening most frequently begins at the posterior part of the apex, and its signs, when not discoverable under the clavicle, may often be found above the spine of the scapula. In tubercle which assumes a chronic form of diffused deposit, it is not uncommon to find the following order: First, one apex; next, the opposite apex; third, the base of the side last attacked. The most chronic is, however, often diagonal, as right apex left base, left apex right base successively. A curious result of some thousand observations may be thus stated: When the observed and customary order of physical signs is reversed, or in any important respect anomalous, the chances of prolonged life are greater. For example, when the base is first attacked and the apex secondarily, the case will be a long one. One anomaly, again, generally implies several, as when softening began at the base, there was often absence of hæmoptysis or of hereditary taint, etc. In a word, the more each case approaches to the ordinary type of the disease, the more rapidly fatal is it sure to be. The conditions which are either antagonistic to tubercle, or which are rarely found in combination with it, were then noticed, and the rule deduced that where any of these are present the case tends to great prolongation. Tubercle seems to monopolize the system. Skin disease (excepting syphilis and the milder rashes, as acne simplex, urticaria, and herpes), external suppurating scrofulous abscess, cancer, gout, tumours, aneurism, and eminently emphysema of the lung are among these. Acute rheumatism after a deposit of tubercle is rare, but not unfrequently precedes it. Of all these combinations that with emphysema tends to the greatest longevity. Persons with dark hair and eyes, although prone to phthisis, generally exhibit it in the chronic form. A freckled state of skin is rarely seen in the consumptive, and the influence of solar light as a counteractive agent was hinted at. Simple anæmia is rarely found in the first stages of the disease; in the later it occurs as a symptom of blood impoverishment. Of tubercular symptoms it may be remarked, that an early profuse hæmoptysis is unfavourable. A single, late, profuse hæmoptysis is often accompanied by relief to the symptoms, and followed by a pause in the progress of the disease. Dry pleurisy (indicated by *frottement*) is theoretically an almost necessary occurrence to insure insulation of diseased parts, and is not unfavourable to prolongation of all favourable symptoms. A quiet pulse and the absence of fever are the most important. Hectic often occurs early before physical signs are present. Its recurrence in advanced cases is invariably associated with either an advance in the stage of existing disease or with a fresh deposit. The wasting of the tissues in consumption has its meaning in keeping the system at a balance with the respiratory power, and a uniform spare habit of body is the most favourable for chronicity. With this is often associated a most valuable condition, which may be called nervous vitality, conferring on the system great powers of endurance.

Dr. Pollock next exhibited a table of the particulars of about 190 cases of phthisis under his care which had already lasted upwards of four years, each case being noted and examined by himself:—

Cases over Four Years.

Males	111
Females	82
Under 20	25
Under 30	71
Under 40	60
Over 40	36
Fist stage	60
Second stage	56
Third stage	71
One lung affected	84
Both lungs affected	97
Hæmoptysis	124
None	65
Have taken cod-liver oil	144
Have not taken cod-liver oil	44
Diarrhœa	39
No diarrhœa	151
Larynx affected	21
Larynx not affected	170
Hereditary predisposition	69
No hereditary predisposition	113
Have taken cod-liver oil three months	82
Have taken cod-liver oil six months	37
Have taken cod-liver oil for years	32
Degrees of waste:—	
First (slight)	97
Second (decidedly thin)	80
Third (extreme)	12
Hectic, marked by sweatings:—	
Now	88
Formerly	30
Not	38

An analysis of the above table is in favour of males.

Advanced age shows increased toleration of phthisis. An individual is not only less likely to contract consumption after thirty-five; but if he has it the disease inclines to the chronic form. The remarkable number of seventy-one are found with cavities, and the emaciation was not in proportion to the stage, for only twelve were in the extreme of wasting. One half had never spat blood, the ordinary proportion of all cases together being about sixty-three per cent. This illustrates the rule of anomalous occurrences referred to above. The absence of hereditary taint is very remarkable. The secondary affections, diarrhœa and disease of larynx, were exceedingly rare. The pressure of hectic was also rare. Finally, the favourable conditions in phthisis may be thus summed up:—

From Physical Signs.—1. Limited quiescent deposit in one lung.

2. Limited well-defined cavity in one lung.

3. The sounds denoting softening becoming drier in a one-sided deposit.

4. The concurrence of emphysema of the lower parts of the lungs with tubercle in the upper.

5. Any unusual localization of physical signs.

From Symptoms.—1. The absence or rarity of fever.

2. The concurrence of skin diseases, external struma, gout, tumours, aneurism, fistula in ano.

3. The lymphatic temperament.

4. A spare habit of body without much variation in the weight.—*Med. Times and Gaz.*, Sept. 17, 1859.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

27. *Three Cases of Tetanus, in which Woorara was used in the Treatment.*—A paper on this subject was read before the Royal Medical and Chirurgical Society (Nov. 22, 1859), by T. SPENCER WELLS, Lecturer on Surgery at the Grosvenor-place School.

The author commenced by stating, that of upwards of 300 cases of ovariotomy recorded, he had only found one in which tetanic symptoms were observed; and referred to the singular fact that tetanus appeared in two out of four cases in which he had performed this operation last month, and to the equally remarkable circumstance that, although he had not seen a case of tetanus for upwards of ten years, a third case had occurred in his own practice within a month, and this after a very trifling operation, proving that the nature of the operation, or the mode of its performance, had little to do with the origin of the disease. The author alluded to recent cases of tetanus treated on the Continent by Vella, Manec, and Chassaignac; and to the experiments of Bernard with the alkaloid *curarina*, the active principle of the poison. He showed that the experiments of Sir B. Brodie in 1811, the application of the results of these experiments by Professor Sewell, at the Veterinary College, and subsequent experiments of Mr. Morgan, and of Dr. Harley, had anticipated all that had been done recently on the Continent, with the exception of making the actual trial on the human subject. The cases now brought forward were the first similar trials made in this country. The author then detailed the particulars of a case of chronic, but severe trismus, going on to opisthotonos, appearing a fortnight after ovariotomy in a patient 41 years of age. The exciting cause appeared to have been a draught of cold air. The great point of interest in the treatment was, that woorara was used hypodermically and epidermically, six grains of the extract having been used in six days. The patient recovered. In a second case, acute tetanus appeared seven days after ovariotomy in a lady, 38 years of age, and proved fatal in three days. The treatment was commenced by assafœtida injections, and woorara was afterwards used, the softened extract having been inoculated in both arms. In the third case, tetanus appeared four days after a simplified perineal operation for the relief of prolapsus uteri in a patient, 51 years of age. It progressed slowly; was treated first by opium and ether, afterwards by woorara, and latterly by chloroform, the influence of which was kept up at intervals for forty-eight hours. The patient died on the evening the paper was read. The author, after expressing regret for the little positive information he had been able to lay before the Society as to the real value of woorara in the treatment of tetanus, thought that he had seen enough to establish the following propositions: 1. That our knowledge of the physiological action of woorara; of its antagonistic effects to the artificial tetanus of strychnine; of the results of its use in idiopathic tetanus of the horse and ass; and the facts that two cases of chronic tetanus in man, on the Continent, and one in this country, have recovered during its application, should encourage further experiments. 2. That although three cases of acute traumatic tetanus in man on the Continent, and two in this country have died, notwithstanding the use of woorara, this should not discourage us from further trials, when we consider the very fatal nature of this form of tetanus, and the fact that in only one of these cases was the woorara applied in a large quantity. 3. That looking to the probable difference in strength of the specimens of extract brought to this country, and to the well-founded belief that they are not all prepared from the same species of vegetable, it would be well, in future trials, to use a solution of the active principle of woorara—the alkaloid *curarina*. 4. That it is desirable to ascertain as far as possible, by experiments on the lower animals, what dose of this alkaloid might be inoculated with safety in man; and whether the artificial tetanus of strychnine establishes a tolerance of curarina. This would afford some test as to the safety of using much larger doses in man when suffering from tetanus than when in a state of health. 5.

That veterinary surgeons should be requested to aid us in our attempts to determine the value of woorara when treating tetanus in the lower animals, by carrying on the dose of the poison until its full effects were shown by suspended animation, and then restoring the animal by artificial respiration. In conclusion, he suggested that more accurate observation is much needed of what may be called the *Natural History of Tetanus*. "It has been demonstrated," he said, "that direct injury to the nerves by pressure, contusion, puncture, or laceration, will induce tetanus. In many cases of tetanus important injuries to nerves have been discovered: such as a knot of whipthong embedded in the ulnar nerve, the knot of a ligature in the sciatic nerve, a splinter of wood in the radial nerve. In other cases, the nerves near a wound have been found reddened and tumefied, and evidences of inflammatory change have extended along nerves from the seat of injury to the spinal cord. But in many cases no change either in the peripheral nerves, or in the spinal cord or brain has been discovered. It is most interesting and important, therefore, to discover whether we may not have two very different conditions confounded under the single term, 'tetanus;' whether in some cases we have spasms dependent upon irritation of a peripheral nerve and increased reflex excitability; and in other cases a blood disease caused by the absorption of a morbid animal poison, developed by perverted secretion of the wounded surface; which poison is analogous in its effects, and possibly in its composition, to urea, strychnine, and the poison of hydrophobia. Of the three cases I have brought before the Society, the two former strike me as instances of tetanus from morbid poison, and the third as dependent on injury to the branches of the perineal nerves. Much light may be thrown on this interesting question, by noting the effects produced on animals by inoculation of the secretions of the wound; or of the blood, or juice of muscle; or of the urine and other excretions of tetanic patients; and I trust that some of the gentlemen now present, may enrich medical science by some such observations as I have ventured to suggest."

Mr. CURLING said that no one who had witnessed the operation for ovariotomy, could be surprised at its occasionally giving rise to tetanus, but he believed that the circumstance of two cases in succession resulting after that operation was quite accidental. A similar occurrence took place at St. Mark's Hospital where there were very many operations for hæmorrhoids, tetanus never having followed them except in two cases which occurred in succession. He agreed with Mr. Wells in thinking that no satisfactory conclusion could be drawn from the two cases he had recorded, as to the beneficial influence of woorara. He remembered the early experiments to which reference had been made, and he had stated in his work on tetanus, that the poison was deserving of a cautious trial in acute cases of tetanus in the human subject. Several cases had come under his care since that period, but the poison was not at hand for him to employ. One important objection to it was the difficulty of regulating its action. It was no doubt deserving of a further trial, but its application should be restricted to genuine cases of acute well-developed tetanus. He had seen many cases recover, not of an acute, but of a severe chronic form. In those cases chloroform had been very advantageous in warding off spasms for a considerable period. It would be a great advantage if a specimen of woorara could be obtained that might be depended on, and if a safe and efficacious mode of exhibiting it could be discovered.

Dr. HARLEY said he agreed with the author in believing that the conflicting opinions regarding the action of woorara on the human body, were chiefly due to the fact of the specimens employed being of ununiform strength. He had in his possession at the present time five specimens of the poison prepared by different tribes; and, although the general action of all was identical, yet their strength varied considerably. There was, however, a second, and equally important reason, namely, the mode of administration. Woorara differed from many remedies in not being absorbed equally readily when introduced into the body by various channels. Thus it could not enter through the unabraded skin, and when taken by the mouth its action was extremely doubtful. Dr. Harley had made a pigeon swallow twenty, and a mouse thirty, times more than was sufficient to destroy them if introduced by a wound, and yet both animals re-

mained unaffected. This did not arise, he said, from the poisons being destroyed in the digestive canal, for he had seen Bernard poison a bird with the excrements of a dog, to which a poisonous dose had been given with impunity. As regards the value of woorara when compared to other narcotics, Dr. Harley remarked that he considered that its superiority consisted in its peculiar power of paralyzing the motory, and not the sensory nerves. Nay more, he said that it could be so administered as to destroy entirely the power of voluntary motion, without impairing the consciousness of the animal. (Dr. Harley related one or two experiments in proof of this statement.) In administering woorara to animals labouring under tetanus induced by strychnine, he therefore gave sufficient to paralyze all the muscles, except those of respiration. In this way he was able to allay the tetanic spasm without destroying the intelligence, or arresting the performance of the organic functions. And by continuing the thus moderated action of woorara until the kidneys had time to eliminate the strychnine from the system, he had been able to save the life of the animal. The theory of its action in tetanus was, Dr. Harley considered, much the same. That is to say, you try to keep the spasms from killing the patient by their violence, until the morbid state calling them into play has exhausted itself. The chances of curing by woorara a patient labouring under strychnine poisoning, are naturally much greater than those of curing a case of traumatic tetanus, where the morbific matter may go on increasing until its effects are more than sufficient to counteract the influence of woorara.

Dr. SIBSON said that in 1838 he wrote to Mr. Waterton on the subject of woorara. Mr. Waterton, after hearing of Mr. Sewell's experiments, went over to New Guinea to obtain a store of the poison, with a view to its use in cases of tetanus or hydrophobia. He afterwards experimented on an ass to show that an animal might be resuscitated, after what would otherwise be a fatal dose, by artificial respiration. He (Dr. Sibson) performed the experiments to try the effect of artificial respiration. For more than an hour the animal lay apparently dead, with no sign of life but the beating of the heart. At length it was made to breathe, and in an hour afterwards was walked round the room by Mr. Waterton. Some of the poison was given to him (Dr. Sibson), and he experimented with it upon a horse, the subject of tetanus. A dose proportionate to that given to the ass did not produce the desired effect; and in increasing it, the animal was destroyed. In another case the animal breathed at the end of three or four hours, but it died before the apparatus could be applied a second time. The animal was free from tetanus during the whole period of the experiment. He had performed experiments on lower animals similar to those described by Dr. Harley. In only one instance was any considerable result produced after giving the poison by the mouth, but in that case he considered that some abrasion had accidentally taken place. In order to overcome tetanus it was necessary to give an adequate dose, and he feared that an adequate dose would be such as absolutely to call for artificial respiration. He had been for years prepared to encounter the responsibility of employing artificial respiration in cases of hydrophobia; and in one instance, in which he was telegraphed for to go to Wakefield, he took his apparatus with him; but, within thirty miles of the town he heard, greatly to his relief, that the patient was dead. He thought the woorara poison offered the best chance of success, from its complete destruction of the disease for at least a number of hours, and he hoped that in some case of hydrophobia it would be tried with artificial respiration. He feared that the symptoms of tetanus would be found too severe to be overcome without loss of life.

Mr. SPENCER WELLS said he should be sorry to see the poison administered in such quantities as to suspend animation, until more favourable results had been produced upon animals than those which followed the experiments of Mr. Sewell and Dr. Sibson. It was encouraging to observe that many of the improvements which were supposed to originate abroad should prove to have arisen in this country. A great stir had been made abroad as to the researches of Bernard and Vella respecting the physiological action of woorara and tetanus; yet years ago Sir B. Brodie, Mr. Sewell, and Mr. Morgan were doing all that the French physiologists and surgeons were now taking credit for. So with regard to the distribution of the sensitive and motor branches of the mixed nerves, which Dr.

Harley gave Bernard the credit of discovering by the aid of woorara—the same thing was pointed out twenty years ago by Mr. Hilton, at Guy's Hospital.

Dr. RADCLIFFE said that in several cases tetanic spasms had given way to conium, which might be administered by the stomach, and was of very certain action. He suggested that this would be a better agent to employ than woorara. —*Med. Times and Gaz.*, Dec. 3, 1859.

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28. *Effusion into the Knee-Joint Treated by Iodine Injections.*—W. COLLES, Esq., Surgeon to Steevens' Hospital, relates (*Dublin Hospital Gaz.*, Dec. 1, 1859) the following example of this:—

"Wm. Frazer, æt. 30, a turnkey, was admitted into Steevens' Hospital, November, 1858. He states that in the year 1850 he received a hurt in the left knee-joint by falling on it whilst making a great effort to secure a prisoner. The joint swelled to a great size.

"He was treated by rest, blisters, and some internal remedies, by which means the swelling was removed in about a week.

"The joint subsequently became swollen several times from very slight causes, but always yielded to treatment till about nine months ago, when he again hurt it severely; it became suddenly swollen and very painful. The pain was much aggravated at night, and accompanied with startings of the limb. The pain ceased in a few days, but the swelling has continued ever since. He could walk with the aid of a stick and perform his duty, but now he was obliged to resign his situation, as the limb was useless from the great swelling and relaxation of the ligaments.

"He was subjected to various kinds of treatment in different institutions without benefit; and on admission into Steevens' Hospital the leg and thigh of the left side were much smaller than the right, but the knee-joint was considerably larger, on account of a large swelling on the inner side of the joint, extending from the tubercle of the tibia to an inch above the patella. The integuments were of a natural colour, and the tumours yielded a uniform soft fluctuating feel, as of fluid beneath.

"There was a small tumour, about the size of half an orange, on the outer side of the joint, also containing fluid, and evidently communicating with that on the inner side.

"The motions of the joint are limited in extent. Different modes of treatment were used, with the object of causing the absorption of the fluid. Mercury was administered locally, blisters, hydriodate of potash, bark, stimulating liniments, strapping, pressure, etc. etc.; these were all successively and assiduously tried without producing any effect on the tumours.

"January 15.—A small trocar was passed into the tumour, and exit given to a fluid of a creamy consistence and yellow colour; the canula became blocked up by flakes of lymph before more than about half an ounce escaped. I did not like to use any efforts to get more away, and was contented to inject about two drachms of the tincture of iodine, which remained in the joint. This caused some uneasiness, which soon passed off. No bad symptoms follow. After waiting three weeks, and finding no alteration in the symptoms, I repeated the operation, using a larger trocar, and evacuated about six ounces of a dark-coloured, but more homogeneous fluid. I injected about an ounce of the tincture of iodine, and could withdraw but a very small quantity of it again; however, its presence did not produce any bad effect.

"In about three weeks the sac became again filled, as large as before the operation. It was again tapped and injected, and the operation was repeated about four times, at intervals of from three to four weeks. After the last operation there was no more fluid secreted, and the joint presented a more natural form. The patient was now able to get about much better, as he could use support to the limb, which felt stronger and more steady under him.

"He soon after left the hospital, as nothing further could be done, as we hoped time would alone suffice to cause an anchylosis in the joint, when all diseased action had ceased.

"He again presented himself this month, November, 1859, one year nearly having elapsed since the operation. There has been no greater swelling; he is

able to go about, not, however, without the aid of a stick, but he feels the limb much stronger, and wears an elastic support to the joint.

"The line of treatment here recorded has been seldom practised in this city, because I consider the cases in which it would be justified are very rare. In general, by perseverance and attention, and more or less frequent changes in the plans of treatment, we at length succeed in causing the absorption of the fluid in such cases as that above recorded; but in the present instance I thought myself justified in resorting to this extreme measure, as the disease had existed for such a long time, was not influenced by any treatment, and the joint had been evidently much altered from the normal structure, and the secreting sac much deformed from ordinary synovial membrane.

"I resorted to the operation not without some fear and anxiety, but I was very soon satisfied as to the propriety of it by the absence of any bad symptoms, and was the more encouraged to repeat the injection till it induced some decided alteration in the disease.

29. *Excision of the Knee-joint.* Dr. EBEN WATSON, in an interesting paper (*Glasgow Medical Journal*, Oct. 1859), states that excision of the knee-joint has been performed eleven times in Glasgow on the living subject. Of these cases only *four* have been successful, and *seven* have been failures. Of these seven patients four died from the effects of the operation; three were submitted to amputation through the thigh, and one of them survived. So that of the eleven patients operated upon, *six* died.

These results may be also expressed as follows:—

Cases of excision, 11 { Total recoveries, 5 { Cures, with useful limbs, 4 / Cure after secondary amputation, 1 } / Total deaths, 6 { After excision, 4 / After secondary amputation, 2 }

or perhaps still more clearly, though somewhat differently, in the following table:—

Number of excisions.	Cases of secondary amputation.	Cures, with useful limbs.	Total recoveries.	Total deaths.
11	3	4	5	6

Mr. Butcher, in his papers on this subject in the *Dublin Journal of Medical Science* (Vols. xix. and xxv., and No. 53), gives, *from many sources*, cases to the number of 83; of these there were 58 cures, and 15 deaths, and 6 successful secondary amputations. One of the 15 deaths was after amputation, and three of the 83 cases disappear from the account, I rather think, because they were too recently operated on to be stated with the others. His account, then, is as follows:—

Number of excisions.	Secondary amputations.	Cures, with useful limbs.	Total recoveries.	Total deaths.
80	7	59	65	15

This is, of course, much more favourable for excision than the former table; but even when the results are combined, the number of deaths is small compared with that of the recoveries. Thus—

Cases.	Total recoveries.	Total deaths.	Secondary amputations.	Cures, with useful limbs.
91	70	21	10	63

These figures show most favourably for the operation of excision, both as to the smallness of the mortality, and as to the satisfactory nature of the result, in

no fewer than 63 out of 91 cases; viz., that the patient retained his two limbs, and was not condemned to hop about for life on one.

The mortality after amputation through the thigh *for disease* is, it must be confessed, higher than that just given for excision of the knee-joint. In the Royal Infirmary of Glasgow, between 1842 and 1857 inclusive, there were performed 128 secondary amputations through the thigh; of that number, 65 patients died, and 63 recovered. About one in two died. But I have not separated the cases of amputation for disease from those for accident in this statement. Now the latter class of cases, viz., secondary amputations of the thigh for accidents, almost all die; but they are not distinguished in the operation-table of the hospital; and, therefore, I could not do so without very great labour, The proportion of deaths given above is, for that reason, greater than if the amputations had all been for disease. I would guess the true percentage at 40 or thereabout.

Now there were only 21 deaths in 91 cases of excision of the knee-joint; and that is 23.07 per cent., or nearly one-third less than the proportion of deaths after amputation through the thigh for disease. It ought to be remembered, too, that these 91 cases were operated on and treated in many different ways, and above all that their diseases and constitutions were different. It is quite possible, therefore, that in future, by a better selection of cases, and a more skilful choice of the precise manner of operating, the mortality may be still further diminished. At all events, it will not do for surgeons any longer to ignore this contrast of results; and, however great their prejudice, however plausible their arguments against the operation of excision of the knee-joint, they must bend to stubborn facts and reconsider their opinions.

Dr. Watson states that he has lately operated by a single straight incision drawn right across from the one side to the other of the knee. Its middle corresponds with the middle of the patella, which I cut out, and open the joint. This is a much smaller wound than that occasioned by either of the other two modes of operating; and it is quite sufficient for the purpose required, namely, of fairly exposing the articular ends of the bones.

In cutting the bones, Dr. W. uses Graham's saw, and slants its blade so as to cut the end of the former convex, and thus save its length while he removes its entire surface. Again, he cut the head of the tibia, so as to make a concavity into which the convex end of the femur fits.

Any of the morbid structures which have not previously been removed, should now be cut off; and finally, the knife should be drawn downwards at each extremity of the wound, so as to facilitate the escape of the discharge. The limb in most cases may now be straightened, and a few wire sutures inserted in the front of the incision. No bloodvessels of importance are cut in the performance of this operation, so that ligatures are seldom required, and almost no blood is lost.

One other point of importance belonging to the operation, has occasioned some difference of opinion among surgeons; and that is, whether or not the hamstring tendons should be cut, when from long disease they have become contracted, or whether they should be gradually extended afterwards. Now, our experience at the Royal Infirmary shows, that the latter process gives far too much pain, and interferes too importantly with the granulation of the wound, to be received as the proper practice. In Case VIII., the worst consequences were attributed by the attending surgeons to the adoption of this proceeding. On the other hand, the cutting of these tendons, already exposed at each side of the wound, could not do any harm, but, on the contrary, would make the future management of the limb a matter of comparative ease to both patient and surgeon.

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30. *Scooping of Bone substituted for Resection or Amputation.*—M. Sédillot, of Strasburg, has for the last two years seized every opportunity, in cases of diseased bone, of scooping out the affected parts, and leaving the cortical portions, rather than have recourse to resection or amputation. In April, 1858, he brought this mode of operating before the Academy of Sciences of Paris, and then stated that he was led to adopt the method from observing the remarkable

osteogenic powers of the periosteum pointed out by M. Flourens and M. Ollier. Instead, however, of dissecting the periosteum from the bone, and removing the latter (a proceeding which offers some difficulty), M. Sédillot leaves the whole cortical portion of the bone, as above stated, and removes the carious parts.

On the 31st of October last the author brought before the same Academy an account of the cases operated upon in this manner. Ten patients recovered, and three died. Amongst the former, M. Sédillot mentions the case of a young girl in whom he had scooped out the lower third of the femur and the condyles; she now walks very well. Another case is that of a young man suffering from caries of the lower part of the left tibia; the scooping here included the whole of the articular extremity, and the inside of the malleolus; the patient now works hard, and can walk fifteen or sixteen miles. The fatal cases may not be charged to the operation; one died of epidemic sloughing phagedena six weeks after the scooping, and the others several months after submitting to operative procedures. M. Marmy, of Lyons, and M. Ehrmann, a military surgeon in Algeria, have both sent to the author a successful case of this operation.

If we are not much mistaken, M. Sédillot's operation has much analogy to the gouging in caries so often successfully practised in the hospitals of this metropolis. But a real improvement in the operation of resection of joints is the careful preservation of as much periosteum from the extremity of the articular surfaces some distance up the shafts as has not been destroyed by the progress of disease. This should be borne in mind by those surgeons who frequently perform resection of joints; nor are the practical proofs of the utility of these precautions wanting. M. Verneuil, of Paris, for instance, has placed several cases of resection of the elbow before the Academy of Sciences, and shows that, by dissecting very carefully whatever periosteum is left, he had, by regeneration of bone, in one of his cases, only two inches shortening, after having removed altogether four inches of osseous texture from the humerus, radius, and ulna. In another case, M. Verneuil was able to leave a regular cylinder of periosteum at the lower extremity of the shaft of the humerus; and in this instance, also, the results were extremely satisfactory.—*Lancet*, Dec. 10, 1859.

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31. *Extra-Capsular Fracture of Neck of Femur.*—Prof. ALEX. GORDON, of Queen's College, Belfast, relates (*Dub. Hosp. Gaz.*, Sept. 15, 1859) the following interesting case:—

"William Mitchell, aged 58, but apparently older, a pensioner, admitted into the Belfast General Hospital, February 12, 1859. He states that his habits are temperate, that for some time past he has been subject to a chronic cough and shortness of breathing. On the evening previous to his admission, when passing along a dark lane, he was tripped by a rope, fell against the curb-stone, and broke his thigh. On admission into hospital, the long splint was applied. On visiting him on the following day, there was no deformity, no shortening of the limb, nor complaint of pain in the thigh or hip.

"15*th*. As the difficulty of breathing had increased, obliging him at intervals to assume the sitting posture, he removed the perineal band to be enabled to sit up. To permit him, therefore, to do so with greater facility and comfort, the long splint was taken off, and the limb placed on the double-inclined plane. This did not cause him pain, at least he made no complaint, nor was the fracture apparently disturbed.

"16*th*. His expression is maniacal; he is talking incoherently. During the night he made several attempts to rise out of bed. The upper fragment of femur can be felt distinctly, about three inches below the trochanter major, causing the thigh at this part to be much bowed outwards. Measurement along the convexity does not show any shortening of the limb, but that of the concavity, or inner side of thigh, is shorter by two inches at least. Pulse 88, soft; skin moist; bowels open; tongue slightly furred; respiration 40. The limb was extended, and a pad and splint applied to the outer side of thigh, which was still kept on the double-inclined plane, and a broad piece of linen, with straps attached, was laid along the outer side of opposite thigh; some of the straps were carried behind, and others in front of the thigh, and being tied, prevented the outward displacement.

"20*th*. Is becoming insensible; the forehead is covered with an abundant perspiration; pulse 108.

"He died on the night of the 21st, the ninth day after the accident; and on the following day the femur was removed from the thigh; much blood was effused amongst the various tissues of the limb, even down to the knee. At first sight, the accident seemed to be a fracture of the upper third of the shaft of the femur, running obliquely upwards to the anterior part of the root of its neck; the capsular ligament attached to its end, preventing displacement upwards; but on removing this ligament, and the other soft parts adherent to the bone, it was found to be an extra-capsular fracture of the cervix femoris.

"When the periosteum was on the cervix, the line of fracture on the fore and upper part of the neck was so indistinct as to be incapable of detection, either by the sight or touch, and it was only by moving the fragments that the whole of its course could be traced. Now, however, when the parts have been boiled, we are able to perceive the full extent and true nature of the injury. The more carefully the case is studied, the more interesting it becomes, as affording us an example of extra-capsular fracture, which may have an influence in settling disputed points, by corroborating the views entertained by some, whilst at the same time it modifies, or is opposed to the statements or conjectures of others.

"The fracture may be said to consist of four main fragments, in each of which may be included several minor ones.

"The first consists of the head and neck. The second consists of the trochanter major; the posterior intertrochanteric line; part of the posterior surface of the lesser trochanter; and a triangular portion of the upper end or the shaft, which may be easily determined by a point on its external surface three and a quarter inches below the base of the trochanters; and from the point just indicated by drawing two other lines, one passing upwards and inwards, and ending where the base of the trochanter joins the neck, the other upwards and backwards to the lesser trochanter. We have thus a portion of the anterior external and posterior surfaces of the shaft attached to the trochanter major and posterior intertrochanteric line. The third portion consists of the lesser trochanter, and a small elongated triangular piece, running obliquely downwards and backwards to the linea aspera. The fourth portion consists of the remainder of the shaft.

"Let us now trace the line of fracture. On looking downwards at the centre of the upper border of the trochanter, where the neck of the femur joins it, impaction commences. The fracture from this point runs obliquely downwards and backwards, internal to the digital fossa; after that it corresponds exactly to the junction of the neck with the posterior intertrochanteric line, and on reaching the upper and back part of the root of the lesser trochanter, it bifurcates, the posterior division running along the middle branch of the linea aspera, whilst the other branch runs above the lesser trochanter, separating it from the neck, and having gone forwards so as to be on a vertical plane anterior to the lesser trochanter, it again divides into a descending and ascending branch. The former running in front of the lesser trochanter, and passing obliquely downwards and backwards joins the branch behind it, or that which is in the middle branch of the linea aspera. Thus we have the lesser trochanter, with a triangular piece of bone attached to it, completely isolated. At the place where the fracture passes above the lesser trochanter, separating it from the neck, there were several comminuted pieces of bone. The fracture after that takes a course upwards and outwards along the inner border of the anterior intertrochanteric line as high as the base of the trochanter. There the impaction ceases, the remainder of the fracture along the upper and anterior part of the root of the neck did not suffer the slightest displacement. Anteriorly, where the intertrochanteric line joins the trochanter, the fracture of the upper end of the shaft commences, whence it runs obliquely downwards and backwards, and joins that proceeding from the lesser trochanter, three and a quarter inches below the base of the greater trochanter. Besides these complete separations, we find partial fractures or fissures, which have, as I shall presently attempt to show, an important bearing on the mode by which fracture of the trochanter is produced. In the upper and external surface of the trochanter major there is a

letter H-like fissure, the upper limbs of which ascend over the upper border of the trochanter, and join the main fracture opposite to the commencement of the impaction, whilst the lower limbs pass half way down the trochanter. The extent of space within these vertical limbs is nearly one-third of the transverse breadth of the trochanter. There is also another fissure in the upper part of the posterior intertrochanteric ridge, which ends in the posterior and superior angle of the greater trochanter.

"Previously to the patient becoming delirious, I was fully satisfied with the statement made by our intelligent house pupil, Mr. Moore, that there was merely a fracture in the upper third of the femur; there was not the slightest appreciable deformity, nor did the patient make any complaint; and even when, in his delirium, he had disarranged the relation of the fractured surfaces, and even when I found the point of the lower end of the upper fragment displaced outwards three and a quarter inches below the base of the trochanter, the thought of the fracture being extra-capsular never occurred to me.

"As the conclusions at which I have arrived from the study of this specimen of extra-capsular fracture are in several respects not in accordance with those of others, I shall first consider the question of shortening of the limb. Dr. Smith says: 'From the opinion, therefore, of Rodet, that there may be no shortening of the limb in certain cases of fracture external to the capsule, I must altogether dissent . . . for in all such injuries there is impaction, and, if so, there must inevitably be shortening of the limb, even though there be no loss of obliquity in the neck of the femur, no separation or displacement of the fractured trochanter, no laceration of the fibrous structure.' (Smith on *Dislocations and Fractures*, pp. 18, 19.) Now, the question at issue is this: Is Dr. Smith right in maintaining that there is, in every instance of fracture of the neck of the femur, external to the capsule, a primary and immediate shortening, and Rodet wrong in supposing that when the trochanter and the fibrous tissue surrounding it is uninjured there is no shortening? I am fully sensible of the difficulty of giving a positive value to the precise signification of the term shortening. If by the words 'primary and immediate shortening,' Dr. Smith means a shortening that can only be detected by the most accurate measurement—a measurement that will detect shortening to the extent of a line, or a line and a half at the utmost—then I should say that this specimen will corroborate his statements. If, on the other hand, he means to imply a primary and immediate shortening, unequivocally capable of being detected in the living subject, and leaving no doubt on the mind of the surgeon that there is shortening—then I say that this specimen will not warrant such a positive statement. On the fore and upper part of the neck there is not the slightest displacement, and even behind and below at the base of the lesser trochanter, where the impaction is best marked, it is not more than a line and a half at the utmost. Now, is it possible that such a slight degree of impaction could cause a primary and immediate shortening capable of detection in the living? I think not. I am, therefore, bound to adopt the following inference, that this is an undoubted example of extra-capsular fracture, in which there was scarcely any eversion of the limb, and no shortening capable of being detected by the most accurate measurement. But, in addition to this, it presents us with an example of an extra-capsular impacted fracture, in which shortening might be referred to two different conditions of the same fracture. The first is that to which I have already referred, viz.: the impaction of the neck of the femur. The second is the oblique fracture through the shaft, which was produced in the following manner: When the lower part of the neck was driven into the shaft by the shock, it became wedged between the posterior part of the base of the lesser trochanter and the inner surface of the shaft, and instead of detaching the posterior part of the base of the lesser trochanter and inferior part of posterior intertrochanteric line, it caused a very oblique and complete fracture of the shaft itself. Although anteriorly and internally the upper part of the shaft is detached from the base of the neck, almost as high as the anterior and internal part of the base of the greater trochanter, and after that running outwards and downwards for three and a half inches, until it meets with that proceeding downwards from the lesser trochanter, still there was no

displacement in the vertical direction, as the attachments of the capsular ligament in front, and the insertion of the gluteus maximus behind prevented it.

"Another interesting question in connection with this fracture is the *modus operandi* of the force causing it. With the first part of Dr. Smith's explanation, I entirely agree, but the latter I cannot adopt. He says (*Ibid.*, p. 17): 'What occurs appears, in fact, to be this—the neck of the femur is, in the first instance, broken by the fall upon the hip, and then driven into the cancellated tissue, between the trochanters, by the weight of the body, and the prolonged action of the shock; but as soon as the neck of the bone is broken, the femur is rotated outwards, and even before the action of the first impulse has ceased. Thus the posterior intertrochanteric ridge being thrown forwards, is forcibly driven against the back of the neck of the femur; two forces, therefore, combine to produce the fracture through the intertrochanteric space, one of which consists in the impaction of the cervix into the shaft, whilst the other is found in the collision which takes place between the broken neck of the bone and the posterior intertrochanteric ridge.' In this, and several other specimens in Queen's College Museum, I do not find that the posterior intertrochanteric ridge lies in contact with the posterior surface of the neck, and, therefore, cannot admit that it is driven forcibly against the back of the neck, and produces the fracture through the trochanter; for if this were the case, would we not find the back of the neck lying against the ridge, and probably a depressed fracture in it? The fracture through the trochanter occurs antecedent to the impaction, and, therefore, antecedent to the inclination forwards of the posterior intertrochanteric ridge; and what seems to me to have occurred, and I think is capable of demonstration in the recent subject, was as follows: the patient, on being tripped, fell upon the posterior and outer surface of the greater trochanter, which was, therefore, thrown inwards and forwards; the impulse, therefore, instead of being received equally on all parts of the base of the neck, fell, in the first instance, on its posterior part at its junction with the greater trochanter and posterior intertrochanteric ridge, which giving way, impaction occurred, and after that the remainder of the fracture. The extent of the comminution and impaction will vary according to the fragility of the bone, and the amount of violence applied. The impaction of the neck will have unquestionably an influence in determining displacement of the trochanteric fragment; but, from the effects of blows on the trochanters, as witnessed on the dead body, the sequence of events seems to be as follows: direct lateral violence produces primary and immediate comminution of the trochanter, fracture of the base of the neck, impaction, additional fractures, and then displacement.

"Violence applied from behind forwards, produces primarily fracture and displacement inwards and forwards of the trochanter, impaction posteriorly, fracture of the base of the neck generally, and then displacement and fracture from the impaction. When the trochanter strikes the ground, it will be for the instant fixed, perhaps at this time, the extremity being in projectile force, acts obliquely, as a lever, in breaking up the base of the neck. I think this specimen points to the impaction acting powerfully at two points, viz.: at the upper part of the neck corresponding to the prominent ridge which bounds superiorly the digital fossa. It is at this part and a little in front of it, that the starting-point of the fracture of the trochanter originates. The second point is, where the neck joins the lesser trochanter. There the compact tissue of the neck in expanding to form the lesser trochanter, is less thick than higher up, and when it gives way and enters the cancellated structure, it drives backwards the base of the posterior trochanter and the intertrochanteric ridge. The impaction thus occurring simultaneously above and below, causes further detachment of the posterior part of the trochanter, even before the broken edge of the intertrochanteric ridge could possibly have come in contact with the posterior surface of the neck. Now, in this instance, the wedge-like action of the base of the neck at these two points, acting as it were differently, and instead of mutually contributing to detach the trochanter, have caused two different fractures. That above is the usual fissure or fracture of the trochanter; that below, instead of acting in a posterior direction on the lesser trochanter and posterior intertrochanteric ridge, has splintered the upper part of the shaft of the femur."

32. *Fracture of the Clavicle between the Coraco-Clavicular Ligaments.*—Prof. GORDON relates the following case of this accident:—

"Sarah Willis, aged 16, a mill-worker, admitted into the Belfast General Hospital, May 31, 1859. She says 'that, whilst at her work, she stumbled, and, falling on her elbow, injured her shoulder.' On looking at her as she lay in bed, the nature of the injury was very evident. The right shoulder was directed more forwards than the left, and, relatively to the clavicle, depressed. The trapezius was visibly projected backwards, opposite to and by the outer end of the inner or sternal fragment of the clavicle, fully half an inch of which projected beyond the inner end of the outer or acromial fragment. The fracture was oblique from behind forwards, and about two lines external to the commencement of the posterior convexity. The acromial fragment posteriorly was three-quarters of an inch in length, whilst along its anterior border it measured one inch. The distance between the acromio-clavicular and sterno-clavicular articulations on the left side was four inches, whilst that of the right or injured side was only three inches. The outer fragment rested against the anterior border of the sternal fragment.

"This is the fourth example of fracture of the clavicle between the coraco-clavicular ligaments which I have met with in a period of six months. Two of them were of old standing, and two recent. The former I had an opportunity of dissecting, and the results have been recorded in the Transactions of the Clinico-Pathological Society. Although two of these instances have occurred subsequently to my first communication on this subject, they confirm fully all the observations then made. I shall now, however, give a summary of the conclusions at which I have arrived, and of the facts observed in the two cases in the living, and of the three specimens deposited in the Museum of Queen's College.

"1. That, from the form of the clavicle, fracture in the posterior concavity, or in that part of it to which the coraco-clavicular ligaments are attached, is not a rare accident, having met with four cases within the short period of six months, two in the living, and two in the dead.

"2. When the clavicle is broken between the coraco-clavicular ligaments, there is generally considerable displacement of the outer fragment forwards on the anterior border of the inner portion; and the attachments of these ligaments do not offer any important resistance to this displacement. If we dissect away all the soft parts, leaving the clavicle attached to the scapula by its ligaments alone, then saw through the clavicle, leaving the compact layer on its under surface untouched, and after that break it, it will be found that the coraco-clavicular ligaments do not offer any appreciable resistance to the outer fragment, being applied against the anterior border of the inner one.

"3. We have fracture between the coraco-clavicular ligaments without displacement, and when that occurs it must be referred to some other cause than to the resistance offered by these ligaments.

"4. The extent and nature of the displacement in fracture between the coraco-clavicular ligaments will be found to be very variable. There may be scarcely any displacement, or, as in No. 1 specimen, the outer fragment may unite almost at a right angle with the inner one, the upper surfaces maintaining the same horizontal level; or, as in No. 2, with the acromial fragment somewhat above the sternal one, and the angle formed between the fragments somewhat less than a right angle; or, as in No. 3, and the case just related, where the outer fragment is displaced so much inwards as to leave half an inch or even a little more of the inner fragment projecting beyond the inner end of the outer fragment. No. 3 presents us with an example in which there was a false joint, the fragments being united by strong ligamentous bands, and, in addition, the outer end of the inner fragments was so rotated by the action of the trapezius muscle, that its upper surface looks forwards and upwards, instead of directly upwards.

"5. The outer end of the sternal fragment seems to be displaced backwards and slightly upwards, but this is more apparent than real, from the shoulder falling downwards and inwards.

"6. From the inclination forwards and inwards of the shoulder, the outer end of the sternal fragment comes in contact with the trapezius; hence, when pres-

sure is made on that muscle opposite the fracture, it becomes applied against the end of the sternal fragment, causing a jagging pain, much more acute than when pressure is made over any other part of the fracture. The severity of this pain, however, will depend on the seat of the fracture, its nature, the form of the clavicle, and the tolerance of the patient to pain. Thus the pain on pressure in my first case, that of the carpenter, was most acute, whilst that of the girl Willis was not complained of.

"7. From the form of the clavicle, fracture occurs most frequently at the centre of the posterior concavity, or a little more external, than between the trapezoid ligament and acromio-clavicular articulations. For, when we take into consideration the short space that exists between the extreme external attachments of the trapezoid ligament and the acromion, the greater transverse breadth of the clavicle here than half an inch or an inch more inwards, and the shorter lever, it is quite evident that fracture between the ligaments and acromion must be a very rare accident, and the result of some peculiar and direct force; whereas, in fracture between the coraco-clavicular ligaments we have a longer lever, lessened diameter, and the curve of the clavicle, all conducing or predisposing to fracture here rather than external to these ligaments.

"8. From the attachments of the coraco-clavicular ligaments, from the three specimens exhibited, and the two cases related, I am of opinion that several of the cases of fracture of the clavicle described by Dr. R. W. Smith, in his *Treatise on Fractures in the Vicinity of the Joints*, as external to the coraco-clavicular ligaments, are really between them, and that his observations on this accident, and those of Mr. Erichsen,[1] apply more correctly to the fracture between than to those external to the coraco-clavicular ligaments."

Professor Gordon presents the following views with regard to the treatment of this injury:—

"The main use of the clavicle is to keep the scapula outwards, and thus preserve the transverse breadth of the shoulder, giving to the upper extremity a greater freedom and scope of action. No matter what may have been the occupation of the individual, the upper surface does not change its form; but such is not the case with its anterior and posterior borders; they become curved just in proportion to the laborious occupation at which the individual may have been engaged. Hence, when the clavicle is fractured, the scapula, no longer held outwards by it, takes the direction which the physiological action of the muscles impresses upon the sound bone, that is, forwards and inwards. The thorax represents a cone, the transverse diameter of which rapidly increases from above downwards; it also slopes obliquely downwards and forwards; and besides its greatest transverse diameter, against which the base of the scapula and a small portion of its venter rest, is not in, but behind its centre. The external two-thirds of the venter and neck of the scapula are not in immediate contact with the ribs, but are held outwards by the clavicle. Now, if we place the scapula, supported by the clavicle, in its natural position, and then withdraw the clavicle, the venter or fossa subscapularis will become more extensively applied to the thorax, and although the posterior border or base of the scapula may still maintain its relative position on the ribs, its external or acromial angle will fall forwards and inwards. Hence, in fracture of the clavicle, this rotation or alteration in the direction of the scapula causes the outer fragment of the clavicle and acromial angle of the scapula to be depressed, though the scapula itself, in its vertical relation to the ribs, has not undergone any change.

"If we observe carefully a person who has sustained a recent fracture of the clavicle, we see that he inclines towards the injured side, with the shoulder seemingly depressed; and if, at the same time, we examine the spinal column, it will present a concavity towards the injured side. If we now direct the patient to straighten himself until the curvature of the spine disappears, and then compare the two shoulders, in most recent cases we shall see that the shoulder of the injured side is no longer depressed, but actually raised; and the more it is elevated, the more will the scapula come into relation with a narrower part of the thorax, and the more, therefore, will its acromial angle fall forwards, inwards,

[1] Science and Art of Surgery.

and downwards, diminishing the distance between the sterno-clavicular and acromio-clavicular articulations.

"The explanation hitherto given by surgeons of the displacements in fractures of the clavicle is, that the outer fragment is drawn forwards and inwards by muscular action, and mainly depressed by the weight of the extremity. That the outer fragment lies beneath the inner one, there cannot be any doubt; however, that it and the scapula are dragged downwards, relatively to the thorax, by the weight of the extremity is a statement the accuracy of which I cannot admit. Is such an explanation in accordance with the recognized influence of muscular contraction in causing deformity? The muscles in this fracture, as in others, are not passive, but active. The weight of the extremity being regarded as the chief agent in causing displacement, the treatment has been conducted so as to counteract it. The lower end of the humerus has been brought forwards, and used as a lever, to force, by its upper end, the scapula upwards, outwards, and backwards, and thus made to counteract the displacement downwards, forwards, and inwards. Have not the form of the thorax and the relations of the scapula to it been overlooked? Has this treatment been successful? All practical surgeons bear testimony that in fractures of the clavicle the treatment has not been very satisfactory. Overlapping of the fragments is the rule, and accurate adaptation the exception.

"The following seems to me to be the nature and cause of the displacement. When the clavicle is broken, the outer or acromial angle of the scapula, no longer held outwards, falls forwards, inwards, and downwards. The form of the thorax conduces to this movement, but the chief agents are the serratus magnus, lesser pectoral, and subclavius muscles; and whilst the scapula is in this position, the rhomboideus levator angulæ scapulæ, and the trapezius muscles elevate it, also assisted very much by the action on the humerus of the clavicular and upper sternal fibres of the great pectoral. The patient leans to the injured side, not to allow the shoulder to be depressed, but to relax the muscles excited to contraction by the irritation of the broken surfaces. To counteract the displacement that occurs, let a large pad, larger than that usually employed, be placed in the axilla, and fixed there by a figure of 8 or clavicular bandage, sufficiently tight not only to fix the pad steadily, but also to keep the shoulders well backwards; let the arm now be extended directly downwards and firmly fixed to the body by a bandage or belt carried horizontally around the body, and permanent extension maintained by passing a band around the perineum or upper and inner part of the thigh (the same as the perineal band used in fractures of the shaft of the femur when treated by the long splint), and extending around the upper end of the forearm, flexed at right angles with the arm.

"When extension is thus made on the arm directly downwards with a large pad in the axilla, the outer fragment passes outwards, and comes into the most accurate apposition with the inner, and this is materially assisted by the form of the thorax, for, as we pull upon the arm, and thus drag the scapula downwards, it is forced outwards and brought over a wider part of the thorax, which counteracts very much the tendency to displacement inwards."—*Dub. Quart. Journ. Med. Science*, Nov., 1859.

33. *Value of Internal Incision in the Treatment of Obstinate Strictures of the Urethra.*—Mr. HENRY THOMPSON read a paper on this subject before the Medical Society of London (Oct. 8, 1859). He commenced by observing that it was not his object to advocate any particular method of treating stricture as the exclusive one, or even as of very general application. In no complaint was there more necessity for exercising a sound discrimination as to which of the various methods sanctioned by experience should be adopted in each individual case. He introduced the subject of internal division of stricture as a mode of treatment which, when properly applied, is one of extreme utility in some of those exceptional cases which had been found not amenable to dilatation. He was prepared to substantiate this statement by the records of a considerable experience. He thought that the many hazardous modes of performing the operation which had been practised at various times and places, but especially on the Continent, had by their bad results naturally raised some prejudice against

incision altogether. But it would be his endeavour to discriminate between these modes, and to point out one which had been proved safe and satisfactory in a very high degree. There are two classes of cases to which the author conceived internal incisions to be applicable: 1. Those in which the stricture is so unyielding that no dilatation, simple or continuous, materially enlarges the passage or ameliorates the symptoms; such examples usually occurring in patients who have been subject to the complaint for twenty years or more, and who have undergone repeated and long-continued courses of treatment. 2. There is a class of cases for some of which, severe and obstinate stricture having occurred in comparatively early life, internal division appeared to be the most appropriate treatment. For these, the object of the operation is not only to remedy present difficulties (as in the preceding class), but also to anticipate future ones, since it is almost absolutely certain that serious, if not fatal injury will result long before the term of life is past, where during the early part of it a severe stricture is established and merely palliative treatment is applied. He believed it to be a question which we are bound seriously to consider, whether it may not be more desirable to cure the patient if possible before extensive urethral disease has been set up, or organic complaints in the bladder or kidneys have been established, than to postpone the attempt until such changes either threaten or have taken place, and the effect of palliative measures has been wellnigh exhausted. This is a view of the subject which, in the author's opinion, has not been sufficiently considered. The manner of performing internal division of the stricture was next described. During the last few years Mr. Thompson had studied the modes generally adopted both at home and abroad, especially the latter, and had made it his business personally to examine and estimate them; the consequence of which was that he had employed the method about to be considered, and with a success, which gave him a high opinion of its utility. For the successful practice of the operation the three following conditions must be complied with: 1. The cutting instrument must be passed through the stricture, and the incision be made from behind forwards, that is, towards the orifice of the urethra, not from before backwards. 2. The limits of the stricture being first accurately defined, the whole of the contracted part should be divided. 3. The borders of the incision should be maintained apart by catheterism subsequently performed, and healing of the incision by first intention be thus prevented. After dwelling on each of these points at some little length, Mr. Thompson invited particular attention to an important point in connection with the kind of stricture to which incisions are applicable. He was anxious to combat a very generally-received opinion that the narrowness of a stricture is the measure of its severity, an opinion which is wholly erroneous and productive of grave error in practice. The truth is that a very narrow stricture—for example, one admitting only an instrument of the size of No. 1—sometimes produces comparatively slight symptoms, and is easily amenable to dilatation, while there are exceptional cases in which a stricture is by no means narrow, admitting say No. 4 or 5, but which is quite non-dilatable, and is accompanied by most severe symptoms. Narrowness, *per se*, is not therefore the gauge of severity. The most important characters of organic stricture, whatever may be its calibre, are non-dilatability and contractility; the former denoting a condition in which the tissue constituting the stricture is so unyielding that dilatation, however carefully employed, does but slightly enlarge its calibre, or improve the symptoms; the latter denoting a quality, through the agency of which, whatever temporary effect may be produced by dilatation, the original degree of narrowing reappears almost immediately after ceasing to employ the instrument. The erroneous belief referred to is exemplified in the remark so frequently met with, that if a No. 4 or 5 can be passed, any incision must be unnecessary. The fact, however, is that not merely the calibre, but many other conditions of the stricture, and also system at large, must be considered before it is possible to come to a conclusion respecting the kind of treatment which should be adopted. These points were discussed in detail by the author. There was one important fact in relation to non-dilatability, viz., that this particular quality of the stricture appears generally to be developed in proportion to its proximity to the external meatus; so that those cases for which incision is most indicated, are precisely those in which the inci-

sion is rarely accompanied by any risk whatever. The instrument which Mr. Thompson considers to supply the best method of fulfilling all the indications required in the majority of cases, is the urethrotome employed by Civiale for fifteen years past, and now slightly modified from the original pattern. The mode of employing it was minutely detailed, and the after-treatment described. For cases in which the stricture is too narrow to be treated by this instrument, Mr. Thompson employs a simple urethrotome of his own design. The accidents which might happen after internal incisions were then discussed and considered from the facts presented in the careful observation of forty-two cases treated by the especial form of urethrotomy described. These accidents were proved to be slight, and in no single instance had led to a fatal, or even threatened a dangerous result. The important fact, said the author, is this, that, in most cases it is in our power by internal urethrotomy to replace a confirmed non-dilatable stricture by a condition of the urethra, in which the occasional use of an instrument maintains the normal calibre. In this manner we may confer upon the patient a benefit of no ordinary value, not only in relation to the removal of present symptoms (which may be accomplished equally by other methods); but in the prevention of those more serious evils, which will certainly arise in the course of advancing age. It is this which has long been an important desideratum in practice; unhappily it has been the custom to regard too much the present results, and too little the future difficulties in our treatment. Most triumphant is dilatation in regard to the present in the great majority of cases, but in a certain proportion of them it does but postpone the evil day. He claimed for internal urethrotomy, when properly applied to appropriate cases, the position of an admirable adjunct or supplement to dilatation. It was on no account to be regarded as a treatment antagonistic to or competitive with dilatation, but as often rendering wholly amenable to that process a stricture which had been completely rebellious to it before. Finally, Mr. Thompson presented the details of eight cases, which he had taken from his own practice as illustrations, each one of which had been treated by him either in the public wards of University College Hospital, or in connection with some medical man, whose name was given as corroborative of the statements made. The results were of the most satisfactory nature.—*Med. Times and Gaz.*, Oct. 29, 1859.

34. *Operation for Artificial Anus in Children.*—A short discussion upon this subject, of some interest, recently took place at the Academy of Medicine. It originated in a communication from M. Rochard, of Brest, who, referring to a former debate, during which some members of the Academy had expressed doubts whether the subjects of operation for artificial anus ever attained the adult age, now adduced five instances in which this had been observed. The reason why M. Rochard has been able to record so great a number of cases seems to be, that since the successful operation by Duret, in 1793, great numbers of children suffering from congenital deficiency of anus have been brought to Brest. The first case referred to by M. Rochard is the above case operated upon by Duret, the subject dying in 1836 only. 2d. A woman, operated upon by Serand, in 1813, is still alive and robust, suffering but little inconvenience from the artificial anus. 3d. A lady operated upon in 1816 is still living at Brest, the mother of four children, and in the perfect enjoyment of life. 4th. A woman, who died at the age of thirty; and 5th, a lad, who died at fourteen, both of causes independent of the infirmity. All these operations were performed by Littré's method. In all the cases there was eversion of the lower end of the intestine. The tumour was only in part reducible, but it was insensible to the touch, and the mucous membrane covering it, in spite of exposure to the air and to contact with the bandages, &c., never became inflamed. M. Robert, who reported upon this paper, agrees with its author in recommending that whenever in a case of imperforate anus no fluctuation can be detected in the ano-perineal region, no operative procedure should be attempted in this direction, but recourse immediately had to forming an artificial anus in the groin. Malgaigne, Velpeau, and others joined in the discussion on the subject, but for this we must refer to the original account.—*B. and F. Med.-Chirurg. Rev.*, Oct., 1859, from *Bull. de l'Acad.*, t. xxiv.

35. *Hemorrhage from the Bowels in Children as a Sign of Polypus of the Rectum.*—Mr. Bryant, Assistant Surgeon to Guy's Hospital, makes (*Lancet*, Nov. 26, 1859) some interesting remarks on this subject, and relates several illustrative cases. The connection between hemorrhage from the bowels in children and the existence of polypus of the rectum is very constant. In the majority of cases which Mr. B. has observed, he says "the disease has existed for many months; they have all occurred in children under ten years, and in most the disease has been regarded and treated for piles. In some cases the discharge of blood from the bowel is constant, and the patient will be brought with its clothes stained, and its buttocks smeared with a bloody mucus. In these instances the polypus will generally be found to be within, if not protruding from, the sphincter. In other examples, occasional discharges of blood will be observed, although not to any very great extent, and this discharge will generally accompany and follow the act of defecation. In others, again, the hemorrhage will take place independently of any such process. There will generally be some straining after stool, but I have never observed any prolapse of the rectum; and although this disease is troublesome to the child, and of course debilitating, by the repeated, if not constant hemorrhage, when once recognized it is easily treated, and rapidly cured."

"The recollection," Mr. B. remarks, "that such a disease is not uncommon, and that it is always associated with hemorrhage from the bowel, should at once lead the surgeon to examine the part with his finger, when the growth will probably be easily detected. It is generally situated about one to two inches up the bowel, and will be found to vary from the size of a pea to that of a large nut. In some cases more than one will be present; they are always very movable, and easily slip away from the finger upon anything like pressure; and, at times, some little difficulty is experienced in fixing them for removal. Such a practice is the only correct treatment, being invariably followed by a successful result. It may be done by means of forceps or ligature; and in many cases I have broken the polypus off its attachment by hooking my finger round its pedicle. No bad result has ever followed. The pedicle is always very slender, although it may be an inch or more in length.

"The structure of the polypus is very simple, microscopically presenting the ordinary characters of the fibro-cellular growths. After removal, no subsequent treatment is required, and recovery may confidently be expected; the rectum, however, should be carefully examined, so that a second polypus be not overlooked.

"I have thus briefly brought this small but not unimportant subject before the notice of the profession, feeling confident that the existence of polypus of the rectum is not so uncommon as is generally believed, and that such an affection is usually mistaken and treated for piles. In children, the presence of bleeding from the rectum should at once lead the practitioner to suspect the existence of a polypus; and, when detected, its removal is the only correct treatment. I have never had an opportunity of seeing a child suffering from piles, and believe that cases so described are generally mistaken, and that, in reality, they are cases of the disease now under consideration."

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36. *Deodorization of Foul Ulcers.*—Mr. Weeden Cooke, at a meeting of the Medical Society of London (Oct. 24), referred to the sulphate of lime and coal tar which has lately been much used and extolled in the French and Italian hospitals for the purpose of cleansing foul and sloughing ulcers. He had himself employed it, and thought that its virtues and advantages were much overrated; that it sometimes produced more irritation in the part than was desirable, and that it was less convenient in application than other equally effective cleansing remedies, whilst it was far inferior in value in sloughing ulcers to that combination of the manganate and permanganate of potash called "manganese cum potassâ." A distinction should be drawn between ulcers secreting unhealthy pus which rapidly decomposed, and ulcers that were foul because of sloughing tissue which would not readily come away. In the former class the chlorate of potash lotion was the neatest and most effective agent in cleansing the ulcers, and rapidly neutralizing the smell by the amount of oxygen which was contained

in the salt. The carrot poultice, the charcoal poultice, and chloride of lime were also very useful in this form of foul ulcers, but none of these was thoroughly effective in foul sloughing ulcers. In these the root of the slough must be destroyed by some caustic application, and this was best effected by the application of manganese cum potassâ upon a small piece of lint the size of the slough. It is very quick in its action; in the course of twenty-four hours the slough generally came away, and with it all the foul odour which had previously been so offensive and injurious, not only to the patient, but to all whose duty it was to be in attendance.

In the discussion which ensued, various applications were mentioned by different speakers, such as charcoal, nitric acid, the tincture of perchloride of iron, lemon-juice, &c. The tincture of the perchloride of iron produced so much pain that the French surgeons in the Crimea had reported against its use. Lemon-juice and nitric acid, diluted according to the circumstances appertaining to the condition and the nature of the sore, were found to be the most efficacious; and Dr. James Bird remarked, that the experience of the French surgeons coincided with his own. When in India, he had found the application of nitric acid, properly diluted, to be the most effective of all remedies, both in hospital gangrene and offensive ulcers.—*Lancet*, Oct. 29, 1859.

37. *Corns on the Sole of the Foot.*—Mr. HOLMES COOTE calls attention (*Lancet*, Dec. 10, 1859) to the treatment of corns on the sole of the foot. The pain attending these is so great that patients are sometimes unable to walk or stand. Mr. Erichsen notices it in his work on surgery. "It is usually," he says, "of small size and round in shape, the neighbouring cuticle being always greatly thickened and hardened. It is extremely sensitive to the touch, the patient shrinking when it is pressed upon, as if an exposed nerve had been injured. On slicing it down with a scalpel, it will be found to be composed of soft, tough, and white epidermis, arranged in tufts or small columns, in the centre of each of which a minute black dot is perceptible. Each tuft appears to be an elongated and thickened papilla, and the black speck is a small point of coagulated blood which has been effused into it. Around the depressions in which each of these corns is seated the hardened cuticle forms a kind of wall."—p. 439.

Mr. Coote has known ulceration to occur in this morbid structure, when a deep and foul sore, excessively sensitive, is formed. It may be healed by rest, but it recurs when the patient resumes the usual habits.

"Now the cause of these corns will generally be found," Mr. C. says, "to proceed from a tense condition of some of the important tendons, that most frequently affected being the tendo-Achillis. When it is so contracted, the foot cannot be raised beyond a right angle; and it follows that the weight of the body is unduly thrown on the fore part of the sole of the foot, where the corn speedily forms. The contraction of the tendon may be so slight as to need careful examination for its detection; but so long as it exists the cause of the corn remains, and it will be found that any other measure, less than the division of the tendon, will be only palliative. The subcutaneous division of the tendon, its elongation, and the restoration of the foot to its normal bearings, must be conducted on the usual principles of orthopedic surgery. The practice has been adopted many years at the Orthopedic Hospital; but it is not so generally known as, in my opinion, is desirable."

OPHTHALMOLOGY.

38. *Intra-Ocular Hemorrhage consecutive to the Operation for Cataract by Extraction.*—JAS. G. HILDIGE relates (*Dublin Hospital Gaz.*, Nov. 15th, 1859) the following case of this accident:—

"Mary H——, aged 65, widow, applied at the Eye Dispensary, Mecklenburgh Street, affected with complete lenticular cataract of both eyes. As the retinæ

were perfectly sound, and the patient's bodily health good, with the exception of slight rheumatic pains, I determined on operating on the left eye by extraction. The lens, which was hard and of a light brownish colour, was extracted without the slightest difficulty, and scarcely a particle of vitreous humour escaped during the operation. It appeared, however, that the friends of the patient had given her a draught of porter previous to the operation, without my knowledge; and about an hour after I had left her, nausea and vomiting set in, a considerable quantity of vitreous humour was evacuated, and when I saw her, the flap of the cornea was protruding between the eyelids, and a portion of the bed-linen was saturated with blood, the space between the lips of the wound being filled by a mass of vitreous humour. Notwithstanding the application of cold lotions, cupping by means of Hurtleloup's artificial leech, &c., the hemorrhage continued for upwards of twenty-four hours, and the patient became so prostrated, that I proposed extirpation of the eye as the only means of arresting the flow of blood. This, however, her friends would not hear of, and the application of cold lotions was continued, her strength being at the same time supported by strong beef-tea, wine, etc. The hemorrhage was eventually arrested by this treatment, but considerable inflammation of the eyeball followed. At the end of three weeks, the patient had partially recovered her strength; the eyeball was, however, atrophied, and vision completely destroyed.

Rivaud-Landrau states that he has only met with this accident four times in two thousand cases of extraction of cataract. In two of these cases, he attributes the hemorrhage to the escape of a considerable portion of the vitreous humour during the operation; and in the remaining two, it was caused by a blow on the eye twenty-four hours after the operation.

"What are the phenomena," he asks, "which present themselves in the globe of the eye when a portion of the vitreous humour is evacuated?" During this movement, the ocular muscles contract spasmodically, which produces minute shocks on the eyeball; the portion of vitreous humour which remains in the deeper part of the globe executes a forced movement forwards, in order to fill up the vacuum. The vitreous humour, in being projected forwards, is detached forcibly from the chorioidea; and it is in this manner that the rupture of the minute sanguineous vessels which wind about the cells of the hyaloid membrane, and radiate from the chorioidea towards them, is produced. Intra-ocular hemorrhage is the immediate result of the rupture of the vessels during the detachment of the vitreous humour from the chorioidea.

Mr. White Cooper, on the other hand, maintains that the detachment of the vitreous humour, instead of being the cause of the hemorrhage, is merely the result of it. According to him, it is the accumulation of blood behind the vitreous humour which, in pushing the latter before it, produces the detachment. In fact, Rivaud-Landrau regards as the principal cause of the hemorrhage what White Cooper considers as merely the effect of it.

In four cases of extraction of cataract at the Ophthalmic Hospital, Moorfields, in which this complication occurred, the eye was extirpated in order to arrest the hemorrhage; and on dissection, a coagulum of blood was found in each case between the sclerotic and choroid coats, the choroid and retina being pushed forwards into the vitreous humour. This would speak rather in favour of Mr. White Cooper's theory.

In a recent article in the *Giornale d'Oftalmologia Italiano*,[1] Dr. Olioli proposes digital compression of the carotid artery on the side corresponding with that of the hemorrhage, as a means of arresting it, instead of resorting to extirpation of the eye. Although he has not had an opportunity of proving the efficacy of this treatment, yet he cites a case of aneurism of the ophthalmic artery in which it was employed with success by Prof. Gioppi, of Padua, and infers from this that the same means might be resorted to with immediate benefit in intra-ocular hemorrhage.

Rivaud-Landrau makes the following remark, speaking of the occurrence of the accident after the extraction of the cataract: "La quantité du sang évacué

[1] Emorragie Intra-oculari od Emoftalmie, e Proposta della Compressione Digitale per Arrestarle. Per A. Olioli, medico-chirurgo oculista esercente in Galliate.

ne peut jamais être assez considerable pour entrainer à sa suite un danger serieux."[1] This assertion is not borne out by the case I have just related; and in another case, operated on by White Cooper, the hemorrhage lasted for thirty-seven hours, and the patient being eighty-seven years of age, serious doubts were entertained of saving her life.

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39. *Night-Blindness, in Connection with Scurvy.*—The *Ophthalmic Hospital Reports* for July, 1859, contains a very interesting paper on this subject by Dr. ALEXANDER BRYSON.

"Night-blindness," Dr. B. remarks, "most unquestionably occurs much more frequently in scurvy than is generally supposed, but in consequence of the simultaneous existence of some more serious symptom, or of symptoms of a less ambiguous character, it frequently passes unnoticed. In fact, in the naval service, patients are generally placed on the sick-list before it occurs, or at all events before it becomes so decided as to induce the patient to complain of it. Still it is almost certain that scurvy occasionally shows itself without any concurrent defect of vision, while there are other instances in which nearly every second case is accompanied by it.

"The following is a remarkable instance of the prevalence of the disease, in connection with scurvy. The crew of Her Majesty's brigantine, 'Griffon,' employed on the west coast of Africa, for the prevention of the slave-trade, had been victualled on salt rations for five months consecutively, when several of the men began to complain that they could not see to do their duty on deck at night, though the moon shone brightly at the time. Amongst themselves it was called moon-blindness, consequently it did not cause much alarm, until out of about fifty white men, twenty-two were affected, and immediately after the sun went down, they had to be led about on the upper deck, in a helpless state of blindness. There was now just cause for alarm, as the vessel with so many men unfit for night duty, was hardly a match for any of the well-armed slavers so common on the coast at that period. Fortunately a man presented himself, complaining of a sore, and a swelling on the calf of one of his legs, the true nature of which there was no mistaking; on examination, his gums were found to be swollen and spongy. He was one of those affected with night-blindness; the other men similarly affected were next examined, as well as those whose vision by night was still good, when it was ascertained that all of the former presented the most unequivocal symptoms of scurvy, and a few of the latter had spongy gums. The officers, five in number, were not attacked either with scurvy or night-blindness, but they had a more varied diet than the men. To obtain fresh meat, fruit, and vegetables, the vessel anchored at Prince's Island, and three days afterwards nearly the whole of the hemeralopic patients had entirely recovered their vision.

"The ship's company of the 'Dolphin,' another African cruiser, had, in the year 1851, been a long time without obtaining any supplies of fresh meat or vegetables; consequently scorbutic symptoms made their appearance, and at the same time there were ten cases of night-blindness. The medical officer mentions that nearly all these men were able to distinguish objects at the distance of three yards by candle-light, and even to read a book held close to the candle; but the instant they went on the upper deck, they gradually began to lose sight of surrounding objects, and had to be led about from place to place. A few could plainly distinguish the stars and the top-gallant masts and yards, but below that plane they could not see anything. Two cases occurred in the 'Dart,' while employed in the Mozambique Channel, for the cure of which blisters and various other means were used; one improved, but the other resisted various modes of treatment, until the vessel anchored in Simon's Bay, and fresh provisions were issued to the crew, when the patient, to the astonishment of the assistant-surgeon, suddenly got well.

"Scurvy made its appearance in several vessels in the Black Sea in the year 1854, and with hardly an exception it was accompanied with night-blindness. In the 'Vengeance' eleven men were put on the sick-list for scurvy, but many

[1] Gazette des Hôpitaux, May 31st, 1859.

others were suffering in a less degree. Eight cases of night-blindness occurred at the same time; some of these patients showed scorbutic symptoms, while others were apparently free from them. None of the officers were affected with either of these diseases, which were simultaneously removed by a better diet.

"In 1855 there were upwards of seventy cases of scurvy in the 'Euryalus,' but two only of night-blindness were noticed. Her Majesty's ship, 'Brisk,' was seventy-two days in making a passage round Cape Horn to San Francisco; consequently during all that time no fresh meat or vegetables could be procured. Before she reached the above port, the surgeon examined the men, and found twelve with scorbutic symptoms; seven cases of night-blindness had occurred during the passage, and four of these were the worst cases of scurvy. They all recovered immediately after they obtained fresh meat and vegetables.

"One of the most remarkable instances of scorbutic night-blindness occurred in the 'Winchester,' while she was returning from India. Twenty cases were entered on the sick-list between Trincomalee and the Cape of Good Hope, but they all recovered when the vessel anchored at Simon's Town, and fresh provisions were issued to the crew; on the passage between the Cape and England, however, when the men had been for some time victualled on salt provisions, the disease reappeared in the same persons, and continued until they obtained fresh provisions at Spithead.

"In convict-ships proceeding to Australia, both scurvy and night-blindness have frequently made their appearance, but the latter often escapes notice in consequence of the prisoners being sent down into prison either at, or a little after, sunset. In the 'Marquis of Hastings," which conveyed prisoners to Hobart Town in 1841, many cases of scurvy occurred, and there were ten of night-blindness, which presented no other symptoms of scorbutic disease.

"In all these, and in many other instances of a similar nature, the appearance of the disease simultaneously with scurvy, and its sudden disappearance when a better diet is obtained—the absence of the disease in vessels in which the men have a sufficiency of fresh meat and vegetables, even though employed in the same localities with others in which it has made its appearance—shows that it is entirely dependent on an improper or erroneous diet."

MIDWIFERY.

40. *Statistical Study of the Causes and Relative Proportion of Stillbirths in Private Country Practice.*—Dr. R. Uvedale West has published (*Lancet*, Nov. 12, 1859) a tabular view of the stillbirths which occurred in 2,962 cases of midwifery, including 36 twin cases, and comprising, therefore, 2,998 children born. No account is taken of abortions. The following is a summary of this table:—

"The total of stillbirths from all causes, as shown in the table given above, is 111 out of 2,998 children born; a proportion of 1 in 27. Of these, 50 were putrid at birth, with various special circumstances noticeable; 8 were cases of craniotomy; 7 were cases in which the death of the child was caused by difficult delivery of the head in footling births, the funis being compressed; in 7 cases, where the funis was prolapsed early in the labour, the death of the child was equally caused by compression of that important organ; 3 deaths are explained by *placenta prævia;* 6 by the severity of protracted labours left to the natural efforts; there were only 2 deaths from severe or protracted labour where instruments were used; in 1 case embryotomy was practised to effect the delivery of monstrously-adherent twins, accounting for the death of 2 children; in 2 cases the ovum was expelled entire before my arrival, and the children suffocated or drowned; 2 of the children exhibited deficient development of the head; 1 was dropsical—*anasarcous* and *ascitic*; 4 deaths are explained by profuse hemorrhage accompanying the labour; 2 by convulsions of the mother during or preceding the labour; in 1 case there was very clear evidence of latent compression of the funis during the labour; 1 death is explained by difficulty of

turning in a neglected case of arm-presentation; in 3 cases the mothers were either moribund or seriously ill; in 3 of the non-putrid cases the placenta was either putrid or diseased; and in only 7 cases it was not possible to assign a cause. Several of the women are shown to be liable to a recurrence of dead children born.

"The ergot of rye is considered by some to be destructive of the life of the child. For that reason I have thought it right to specify in every case, even in the putrid ones, whether the ergot was or was not given. It results that the ergot was given in 24 cases out of the whole number of 111 children born dead. But as the vast majority of the deaths where ergot of rye had been given are explicable from other causes, I think we may acquit this much-abused drug, and especially so if we study the last list of cases—that, namely, where the 'cause was not manifest.' Let me here explain that I have given the ergot of rye 201 times out of the last 800 cases I have attended. This is a frightful proportion; but never mind. My register stands at No. 2,962 at the present date. If the list of 'cause not manifest' be now looked at, it will be seen that the last 4 cases in that list fall within the 800 cases alluded to. The ergot of rye was given in only 1 of those 4 cases; precisely the proportion of ergot of rye for the whole number of 800—1 in 4.

"I have thought it important in the study of the causes and the frequency of the mortality of children during labour, to insert in my table the number of each case. It may thus be seen at a glance how far the inexperience of a young practitioner may contribute to an increase of such mortality. Let us, for example, examine the numbers of the cases of craniotomy. It will be seen that nearly all the cases that I was exclusively responsible for, fall within the first 1,000. How far a gradually acquired ability in the use of the vectis and forceps[1] may have enabled me to discard this *wilful murder* from my practice, I leave to the judg-

[1] It may assist the reader in the appreciation of my argument if I here state briefly that I delivered, with the one or the other of those instruments, in the first 1,000 cases, 4 times only; in the second 1,000, 16 times; and in the third 1,000, now nearly completed, 77 times, with the result, as regards the life of the child, shown in the text. And the following extract from the index to my register will illustrate, though, perhaps, more feebly, the advantage of artificial assistance in other circumstances of danger to the life of the fœtus than mere protracted labour:—

FUNIS PROLAPSED; in 19 cases; in which,

The child born dead: 11 cases, viz.:—
Head-presentation; left to nature; 168 B., 726 G., 2,400 B.
" " forceps case: 2,301* B.
" " difficult turning case: 2,683 B.
Feet-presentation: 645, 1,916, 2,057, 2,065, 2,228. Of which,
Child putrid: 1,916 G., 2,057 G., 2,228 G.
Placenta prævia: 645 B.
Arm-presentation; turning performed: 69 B.

The child born alive: 8 cases, viz.:—
Head-presentation; left to nature: 1,667 B., 2,345 G.
" " vectis used: 1,643 B.
" " turning performed: 2,504 B., 2,553 G., 2,579 B.
Arm-presentation; turning performed; 2,281 G.
Feet-presentation, with placenta prævia; 1,316 G.

CONVULSIONS OF THE MOTHER,

Preceding or accompanying the labour: 2,191, 2,320, 2,729. Of which,
The child born dead; 2 cases, viz.:—
Labour unassisted: 2,191* B., 2,320* B.
The child born alive; 1 case, viz.:—
Delivery with vectis: 2,729* G.

It is very probable that, although the mere expulsion of the child might have been easily effected in cases 2,504, 2,553, 2,579, and 2,729, if they had been left to nature; yet the children would have been all born dead, if artificial delivery had not been performed.

ment of my readers, pointing out at the same time that *pari passu* with what I may call the *abolition of craniotomy*, or, at any rate, the greatly diminished frequency of the practice, the fœtal mortality from unassisted protracted labour disappears also from my practice."

41. *Statistical Report of Thirteen Hundred Midwifery Cases attended in Private Practice.* By Anderson Smith, F. R. C. S.—Of 1,320 children, 700 were males, 620 females. 1,255 were born alive; 65 dead—of these, 24 were putrid. Premature births numbered 38; twin cases, 20; of these, 35 children were born alive, 1 dead, and 4 putrid. Malpresentations and positions were 58—namely, forehead towards the pubic arch, 20; face, 2; breech, 23; of these, 14 children were born alive, 7 dead, and 2 putrid. Lower extremities, 8; 4 alive, 3 dead, and 1 putrid. Arm, 1 alive. Compound, 4; 1 alive, 3 dead. Prolapse of the funis happened in 6 cases, in all of which the children were stillborn. Accidental hemorrhage occurred 14 times; in 10 cases the children were born alive, in 4 dead. 3 cases of complete placenta prævia, the children stillborn; 2 of partial placenta prævia, 1 child alive, the other dead; 6 cases of slight post-partum hemorrhage; 2 of epileptic and 1 of hysterical convulsions; 3 of peritonitis; 1 of mania. Version was had recourse to in 4, the forceps in 25, and craniotomy in 3 cases. The average duration of labour in primiparæ was 18½ hours; the shortest at the full time, 2¼ hours.

Remarks.—Of the 41 stillborn, death in 29 cases was the result of malpresentation, accidental or unavoidable hemorrhage, prolapse of the funis, or craniotomy. Of the remaining 12 cases, in 1 instance the funis was prolapsed and pulseless before the expulsion of the shoulders; 2 children made ineffectual efforts to respire; in 1 case, the mother had at her previous confinement been delivered of a putrid fœtus; 2 were the children of mothers suffering from disease of the heart; 1 was the second of twins; and the remainder were mostly prematurely born.

In the 2 cases of partial placenta prævia, I merely ruptured the membranes, and the labours terminated naturally. Of the 3 of complete, in two instances I tore through the placenta, turned, and brought down the feet. In the third, I was pursuing the same treatment, when a violent pain expelled the placenta, and, although the hemorrhage then ceased, the patient died on the fourth day; and I may here state that this is the only instance in 1,300 cases in which the mother has died from any cause connected with parturition.

Not once has there occurred a case either of retention of the placenta or severe post-partum hemorrhage. Immunity from both these complications I attribute mainly to two causes—namely, the practice of applying a binder as soon as the second stage of labour commences, tightening it directly after the birth of the child, and again after the expulsion of the placenta, and abstaining from the administration of ergot. By the former the regular contraction of the uterus is effected, and by the latter its irregular contraction prevented.

The number of forceps cases appears great, but, as far as my experience extends, no bad consequences have resulted from their use; and I am inclined to believe that laceration of the perineum is rather prevented than caused by their timely employment.—*Lancet*, Nov. 12, 1859.

42. *Statistics of Midwifery from the Records of Private Practice.*—Dr. Dunn read before the Obstetrical Society of London a summary of his midwifery records for twenty years. He began by expressing his conviction that the records of private practice might be usefully and advantageously contrasted with the statistics of lying-in hospitals and public institutions; and that while his own experience could only be brought to bear upon the working and middle classes of society, he hoped—seeing how important was the influence which the different modes and habits of life had upon the parturient process—that other Fellows of the Society would not be wanting to supply the desiderata in relation to the other grades and ranks of social life; not only to the highest, to those living in the lap of luxury, surrounded by the elegances and enjoying all the indulgences of life, but also to the lowest, to those sunk in the depths of indigence, ignorance, and penury, and often without even the ordinary comforts of life. The author

considered that what had been said of statistics in relation to medicine in general, applied with peculiar force to obstetrics in particular; for what we wanted in midwifery "were facts, comparable facts, numerous facts, well observed, carefully arranged, minutely classified, and acutely analyzed." From 1831 to 1850, a period of twenty years, he had registered 4,049 cases of midwifery as occurring in his practice. Of these, after deducting 228 for premature births, there were 2,133 male, and 1,688 female children. In regard to plurality of infants, there were 2 cases of triplets, and 45 of twins. He had met with 3 cases of monstrosity, one of which was worthy of notice, and had been put upon record in the *Lancet* for April 27, 1844. It was that form of monstrosity which Dr. A. G. Otto has designated *Monstrum humanum sereniforme*. He had met with one instance of the hydatidiform or vesicular mole, and several cases of cranial blood-swellings, 3 of hare-lip, 4 of cleft palate, 3 of spina bifida, and 5 of imperforate anus. In one of the last, Amussat's operation for artificial anus in the left lumbar region was attempted, but was not successful—the descending colon was found to be impervious and not larger than a crow-quill. There were 170 stillborn children in all, from various causes, and in 30 cases death was attributable to the pressure of the cranial bones upon the brain in tedious and difficult labours. There were 60 instances of preternatural presentations. Of 11 cases, in which there was a prolapsus of the funis, 8 were born dead; and of these, in 3 instances the cord came down with the head, in 2 with the head and arm, in 1 with the foot, and in 2 with the shoulder. Of 25 breech-presentations, 9 were stillborn; and of these, 5 were putrid. There were three face-presentations, 1 child dead; 11 cases face to pubes; 2 head and arm, both dead; 3 hand, and 3 footling cases. In the use of the forceps the author confessed to have had but slender experience. Impressed with the importance of the maxim, that "a meddlesome midwifery is a bad midwifery," he had always, in the absence of danger to the life of the mother, and when convinced, in his own mind, that the natural efforts would effect delivery, been content to wait, and had avoided instrumental interference. He had had ten cases of craniotomy; two proved fatal to the mothers—one from sloughing of the bladder, the other from a tumour at the neck of the womb. Once satisfied that the child was dead, he had never hesitated to have recourse, without delay, to craniotomy. The use of the stethoscope in such cases he considered of paramount importance. Of placenta prævia, six cases had occurred in his practice; three since the promulgation of Dr. Simpson's views and mode of treatment. He gave a brief narrative of two of these; one as having presented evidence to his own mind that the detached portion of the placenta, from its appearance and condition, had afforded the channel through which the blood had gushed; and the other as being an instance of the instantaneous arrest of the hemorrhage as soon as the placenta had been entirely and completely separated from the uterus. He had met with thirty cases of adherent placenta requiring the introduction of the hand into the uterus; and four instances of the hour-glass contraction. He had witnessed two fatal cases from sheer exhaustion after delivery, where the hemorrhage before the birth of the child had been great; and one fatal case of internal flooding. Other fatal cases of exhaustion he referred to, which were unconnected with the loss of blood. One interesting and instructive case had come under his notice, in which, while the mother lay in a state of coma from an apoplectic seizure, and the phenomena of life were reduced to a mere series of automatic movements, a fœtus of five or six months was expelled from the womb. Of twelve cases of puerperal fever which had occurred, three were acute, and terminated fatally. In one of these, the placenta had been found adherent, with hour-glass contraction of the uterus, and great hemorrhage. In all, excepting where an hereditary tendency existed to mental disease, the loss of blood had been great. Of puerperal convulsions, he had met with four cases—none fatal; of phlegmasia dolens, six, and two proved fatal; of scarlatina, three, and one died.

Dr. Tanner remarked that the ruling law in Dr. Dunn's practice seemed to have been the old proverb, that "meddlesome midwifery is bad." Dr. Tanner considered that this rule was the cause of a great deal of mischief. In practice it was not only necessary to consider the life of the mother, but also to how great an extent we might beneficially mitigate her sufferings. A lingering labour

could hardly be otherwise than injurious to both mother and child. Although his practice had been much smaller than Mr. Dunn's, yet during the last twelve years he had employed the forceps much more frequently than this gentleman, and had obtained only the happiest results from such a proceeding. He had never found the slightest mischief result from it either to offspring or parent.—*Med. Times and Gaz.*, Nov. 12, 1859.

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43. *Sickness of Pregnancy.*—Dr. C. E. Bagot calls attention (*Dublin Med. Press*, Oct. 12, 1859) to the employment of calomel pushed to slight salivation as a most successful mode of relieving this sometimes intractable and dangerous affection; a practice which he recommended in 1846. In that year he had under his care a woman labouring under that extreme form of sickness from pregnancy which placed life in the most imminent jeopardy. I had tried all the usual remedies suggested in such cases, and found them one after another to fail in producing any relief. Although there were no symptoms whatsoever which would make me suppose that any inflammation was either the proximate or remote cause of the sickness, I resolved to try the effects of mercury, and having had some experience of the powers of calomel in allaying other forms of vomiting, I fixed on the administration of this preparation, steadily persevering in its use until her gums showed appearances of salivation, which they did in a very short time. This treatment resulted in the best effects. Immediately that slight salivation took place, the vomiting, previously so persistent, at once ceased, food remained on the stomach, the patient rapidly recovered, and was in due time safely delivered of a full-grown infant.

The sickness of this patient, Dr. B. says, was of the very worst form, her symptoms were so urgent that he despaired of her existence being prolonged; her prostration of strength was excessive; her emaciation extreme; her pulse a small thread; she had no tenderness in the epigastrium; neither had she pain in the region of the womb, nor the least uneasiness on pressure over that organ; she had no febrile nor inflammatory symptoms, and yet the most complete relief followed the exhibition of the mercurial pushed to slight salivation.

In two subsequent pregnancies this patient suffered equally from the same urgent symptoms, and on both occasions she was relieved by the same medicine.

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44. *Normal Hypertrophy of the Heart during Pregnancy.*—In our number for July last (p. 276) we gave a brief notice of the researches of M. Larcher on this subject, and shall now furnish a fuller account of them.

Mr. Larcher has had ample means of investigating this subject at the Paris Maternité, for he has examined 130 pregnant women, the greater number of whom succumbed to puerperal fever—no lesion having preceded or given rise to the condition of the heart observed in them. The conclusion he comes to is, that the *heart is normally in a state of hypertrophy during gestation.* The walls of the left ventricle become increased by at least from a fourth to a third in thickness, its texture being also more firm and its colour more bright—the right ventricle and the auricles retaining their normal thickness. These observations, made by M. Larcher, date back some thirty years, and have been confirmed by subsequent ones, made with great exactitude, by M. Ducrest, upon 100 other women: but why this paper has been so long in being published no explanation is given.

Within certain limits this condition of things may co-exist with the maintenance of health; but it none the less may be taken to express a predisposition to congestions and hemorrhages. If, as the general rule, the hypertrophy gradually disappears after parturition, it may be otherwise in exceptional instances, especially where the recurrence of pregnancy has been frequent, and with short intervals. Is this not a cause of the varied lesions of the circulatory apparatus so commonly met with in women who have borne many children, either at too premature an epoch, at too brief intervals, or during an unfavourable condition of health? There is every reason, too, to believe that the bronchitis, which is so common during pregnancy, derives much of its character of persistency from this condition of the heart. Again, may we not attribute to this the greater danger of pneumonia when developed in pregnant women, and the frequency with

which abortion then occurs? The various forms of hemorrhage met with in pregnancy, as epistaxis, hæmoptysis, metrorrhagia, and apoplexy, are likewise predisposed to by this hypertrophy, normal though it be. Although pregnancy may, in the majority of cases, suspend or render slower the progress of pulmonary consumption, the progress of this affection becomes accelerated after delivery, and the still hypertrophied heart increases the perturbation of the respiratory apparatus.—*Med. Times and Gaz.*, December 3, 1859, from *Archives Générales*, tome xiii.

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45. *Inverted Uterus Replaced after a Lapse of nearly Twelve Months.*—Dr. CHARLES WEST reports (*Med. Times and Gaz.*, Oct. 29, 1859) the following case of this:—

A. A., aged 25, applied at the out-patients' room of St. Bartholomew's Hospital, August 27, 1854, when she gave the following history of herself:—

She had been married five years, and had given birth to two children, of whom the former was born after a natural labour two years and a half since; the second on October 16, 1858. The child in this instance also was born alive after an easy labour, but the placenta was retained for three and a half hours, during which time very great hemorrhage took place, and in consequence of it the patient became insensible, and was, therefore, unable to say whether it was eventually removed by hand, or expelled by the natural efforts. She was left by her labour in a state of such extreme weakness that she was quite unable to suckle her child; and suffered in addition from much pain in the abdomen and diarrhœa. These ailments confined her to bed; and at the end of five weeks phlegmasia dolens of the right leg came on, for which leeches were applied, and other treatment was adopted, until, at the expiration of seven weeks, she sought admission into the London Hospital, where she remained for a month, and left the hospital much benefited as far as that ailment was concerned.

Soon after leaving the hospital, and about three months after confinement, the menses first reappeared. From the first they were profuse, and intermingled with coagula; they lasted longer than natural, and returned more frequently, and for some time she had completely lost count as to when her periods were due, so frequent was their return, so almost constant their presence, while an abundant yellowish leucorrhœa appeared immediately on the cessation of the sanguineous discharge. The return of the hemorrhage compelled her on each occasion to keep her bed; but in spite of this precaution she had been reduced by it to a state of the most extreme exhaustion, her skin was sallow, her pulse very feeble, and very frequent, and she had the aspect of a patient suffering from advanced malignant disease.

On making a vaginal examination, a tumour of an oval form was discovered hanging down for about two inches and a half through the os uteri, which closely surrounded, but did not constrict it. Suspicion was raised as to its nature by finding that the pedicle of the tumour was of the same thickness as its extremity, and also by the fingers when passed up behind it encountering a *cul-de-sac*, as if the uterus, with the exception of its orifice, were inverted. Hemorrhage was not excited by the examination, but a rather abundant blood-stained purulent leucorrhœa. The patient was at once admitted into the hospital, and on the 29th the diagnosis was established by the following means:—

1st. The uterine sound carried round the pedicle of the tumour encountered resistance to its further passage all round at the distance of half an inch. The finger, though introduced easily behind the tumour, could not be passed in front of it, as the anterior lip was too closely in contact with it.

2d. The fingers introduced into the rectum could without much difficulty be carried above the fundus of the tumour, showing that the body felt per vaginam was not an out-growth from the uterus, but the uterus itself in an altered position.

3d. If while one hand was in the vagina the other was pressed firmly over the symphysis pubis; at first, no body was felt between the two hands. Pressure made against the tumour in the vagina, however, brought it before long within the grasp of the other hand, when it was possible, through the thin abdominal walls, not only to distinguish its contour, but even to perceive the

circular depression in its upper part which indicated the point of inversion of the womb.

Dr. Tyler Smith's case[1] suggested an imitation of his proceedings, and, accordingly, after efforts made with the hand by grasping and compressing the womb to restore its position, or at least to render it more yielding, an air-pessary was introduced into the vagina and inflated to as great an extent as the patient could bear. On the following morning the os uteri was found much more dilated, and its tissue much more yielding, so that the fingers could now be passed all round the tumour with ease, and everywhere discovered the inversion of the substance of the womb. Manipulation of the inverted uterus was repeated daily until the 5th of September, and the pessary was on each occasion reintroduced and reinflated, with the exception of one day, on which it was discontinued, in order to obtain relief for the bowels.

It did not seem, however, that much was gained by the proceedings beyond that increased dilatation of the os which was obtained by the first introduction of the pessary. The vaginal walls, indeed, were rendered more yielding by the extension to which they had been subjected by the pessary, so that the whole uterus admitted of being pushed up in the pelvis more readily than before, but no change was effected in the relation of the inverted body itself. A very offensive leucorrhœa had been excited by the pessary, and its distension had occasioned a very painful stretching of the vagina; but in spite of this the patient's health was already much better than at the time of her admission; she had already gained strength, and her complexion had lost something of its sickly hue.

I now attempted to modify the instrument, and had a pessary constructed so as to expand at its upper third more than elsewhere, in order to avoid needless stretching of the vagina; while it was fitted, by means of a wire stem, to a girdle which encircled the body in order to obtain a fixed point from which the pressure should act; a condition altogether absent in the air-pessary as at first employed.

This was first tried on September 20, the patient having remained since the 5th without any attempts at the replacement of the organ. Some advantage seemed to be gained by the pessary thus modified. It retained its position well, and seemed to produce less discomfort, and to cause a less profuse and less offensive discharge. Still it did not appear to exert any influence on the uterus itself, its force being rather expended in stretching the vagina.

After a few days' trial, I accordingly removed it, and had another constructed of smaller dimensions, under the impression that if it were introduced within the os uteri and there inflated it was more likely to expand the uterine walls, and thus to replace the organ than by any mere pressure exerted from below upwards against the fundus of the womb. On October 3, this new apparatus was introduced for the first time, and though no effect was produced in the first twenty-four hours, it was reintroduced on the 4th, and allowed to remain for forty-eight hours in its position. On the 5th, the patient complained of a good deal of pain in the abdomen, though not of more than she had experienced on some former occasions; and it was with a feeling of glad surprise that, on the 6th, it was discovered that the organ had resumed its natural position.

The os uteri was widely open so as readily to admit two fingers, and its lips were much swollen, the uterine sound passed nearly three and a half inches, and the womb was now felt in its natural position by the hand placed over the pubes. The patient was kept quiet in bed, and, for the next twenty-four hours, the urine was drawn off by the catheter. On the 7th, the sound ascertained that the womb still retained its proper position. Menstruation came on on the evening of that day, and continued scantily until the 11th; and on the 13th, the sound discovered the uterus to have somewhat contracted, and now to measure scarcely three inches; and on the 18th, the patient left the hospital apparently in perfect health, and having walked about for some days without inconvenience.

Dr. West makes the following remarks on the best mode of employing the

[1] See number of this journal for July, 1858, p. 270.

air-pessary, which may be of use to those who hereafter meet with a similar case:—

"1st. Neither in this case nor in another which came under my care, some ten years ago, did any benefit appear to result from manipulation of the womb, or from any attempts with the hand to replace the organ, or to reduce its bulk. The utility of such endeavours will, I apprehend, be limited to instances of recent inversion, or to those exceptional cases in which the womb remains comparatively yielding and flaccid some weeks after the occurrence of the accident, as it appears to have done in those reported by Dr. Belcombe,[1] and Dr. Miller.[2]

"2d. Many inconveniences attend the employment of the common air-pessary, partly from the want of some fixed support to retain it in its place, and partly from the circumstance that the pressure it exerts being equal in all directions, a most painful distension of the vagina is inseparable from any attempt to exert efficient counter-pressure against the inverted womb.

"3d. This disadvantage may be easily overcome by means of a belt to fasten round the waist, the anterior half of which, made of steel, serves as a fixed point for a metallic wire, which is attached to a small wooden disk or cup that bears the pessary. The pessary itself, made of vulcanized India-rubber, and in this instance four inches long by five in circumference at its middle, was rendered comparatively inelastic at its lower half by the introduction of several layers of linen between the folds of the India-rubber, while no such material intervened to prevent the full expansion of its upper half when it was filled with air by means of the syringe through the elastic tube that was connected with it. By this means continued pressure was exercised against the fundus of the inverted uterus, without painful distension of the vagina.

"4th. I am uncertain as to the exact mode in which the replacement of the uterus is effected, and doubt whether it is due to the direct pressure of the pessary against the fundus of the uterus, so much as to the unfolding of the uterine wall by the instrument when introduced into the shallow *cul-de-sac* within the os uteri, formed by the still uninverted portion of the cervix. If this supposition be correct, one's endeavour in any future case would be, first, to introduce a small pessary within the os, in order to dilate the aperture, and to follow this up by the employment of one somewhat larger, with the view of thus unfolding the wall of the organ, rather than to force the fundus upwards by direct pressure against it."

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

46. *Poisoning with Sulphocyanide of Potassium.*—Dr. SETSCHENOW, in a paper on this subject, supplies the following inferences as to the action of the sulphocyanide on animal bodies; in his experiments he used frogs as the animals to be operated on, except in one instance, when a rabbit was selected.

(*a.*) The sulphocyanide introduced into the stomach acts as a poison; causes decrease of the muscular irritability, and destroys life.

(*b.*) The sensibility of the skin in poisoning by sulphocyanide, either by introduction of the poison into the stomach by the mouth, or by injection of it under the skin, decreases more quickly than the motive power of the animal.

(*c.*) Brought into contact, in solution, with muscular structure, it does not produce direct paralysis of muscle.

(*d.*) The symptoms induced by this salt when introduced into the body, prove that the action of the poison is primarily exerted on the brain, then upon the spinal cord, and through these nerve centres on the organs of locomotion and sensation.

(*e.*) When the heart has ceased to beat, its action may be excited for a time

[1] Medical Times and Gazette, 1841, vol. vii. p. 783.
[2] Edinburgh Monthly Journal, December, 1851.

by external irritation; but this action or irritation ceases much sooner in the heart than in the voluntary muscles in cases where the poison has been inserted under the skin. On the contrary, in cases where the poison was introduced by the mouth, the heart, even to the period of three hours after the administration, and when the animal was reactionless (*reactionslos*), may be excited to contraction. The author, therefore, infers that there is a difference between the two modes of poisoning. In most instances the heart ceases to act during the diastole, and is left full of blood.

In one instance noted by Dr. Setschenow, he injected a portion of concentrated solution of the sulphocyanide into the external jugular vein of a rabbit. Death was almost momentary, with convulsions. A quarter of an hour after the operation the heart was found rigid, and the muscles of the neck and of the hinder extremities were also found rigid. In this example it was admitted by the experimentalist that by the direct action of the poison on the muscles, their paralysis and death result. In this respect, therefore, Bernard's view is admitted as correct; but Setschenow qualifies the admission by trying to prove that there is a difference between absence of irritability of muscular fibre and actual death.—*B. and F. Med.-Chirurg. Rev.*, Oct., 1859, from *Archiv fur Path. Anat. und Phys.*, von R. Virchow, Sept., 1858.

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47. *Poisoning by Cyanide of Potassium*.—Dr. A. Schauenstein, in a communication on poisoning by cyanide of potassium, gives an account of five cases of death by the cyanide. The author, who is a judicial chemist, comments upon the great increase of deaths by suicide through the agency of this poison. Thus, in Vienna, from 1851 to 1856, only two poisonings were noted, one of which was doubtful; while from August, 1857, to December, 1858, no less than five cases came under the personal observation of the author. In proportion to this increase of deaths from the cyanide there was a corresponding decrease of deaths from arsenic.

Dr. Schauenstein relates at length three of the cases observed, and in brief the pathology of the two others. In all cases the death seems to have been sudden. In one case, in a young girl, strong tetanic spasms came on directly after the poison had been taken, and death took place in less than an hour. In the second case, occurring in a young man, death took place almost instantly, and with no striking symptoms. The third case was similar; no note of the symptoms in the remaining two cases is given, but Dr. Schauenstein observes, that in several of the cases death took place suddenly, as in apoplexy.

In all the cases a post-mortem examination was conducted, but the appearances observed are considered by the author as offering nothing very characteristic. They were:—

(*a.*) The brain containing more or less blood.

(*b.*) The blood in the cavities of the heart dark, and of thick consistency.

(*c.*) The condition of the stomach various. In one case the mucous surface presented no particular colouring. In the case where life was prolonged nearly an hour the mucous membrane was slightly red, but offered no other extraordinary appearance. In another case, the death being very sudden, the mucous membrane was of a dark red colour, swollen, and in places covered with numerous bloody points; the contents of the stomach were also of blood-red colour; the two remaining cases of the five presented similar appearances in a less degree.

(*d.*) The smell of prussic acid in the stomach was very evident in four of the cases. But in one case, on account of the quantity of undigested food in the stomach, the smell remained hidden entirely.

(*e.*) The reaction of the contents of the stomach was strongly alkaline, and in every case chemical research proved without doubt the presence of prussic acid; but constantly formic acid was also found, showing that prussic acid in the stomach is transformed into formic in many cases.

[This latter fact, one of great interest, was originally pointed out by Dr. Schauenstein in the "Wochenbl. der Zeitschrift der k. k. Ges. der Aerzte, No. 3, 1857."]

Dr. Schauenstein, in commenting on the cases, opines that there are no true

and distinguishing pathological indications by which the effects of the poison can be safely pronounced. He further observes that the chemical detection may become equally difficult in instances where, from the body having been dead several days, or having undergone a rapid decomposition, the poison has been decomposed.—*B. and F. Med.-Chirurg. Rev.*, Oct., 1859. from *Zeitschrift der k. k. Gesellsschaft. der Aerzte zu Wien*, No. 1, Jan. 3, 1859.

48. *Action of Strychnine and Nicotine.*—In two papers, published in the *Proceedings of the Royal Irish Academy*, November 29, 1856, and June 28, 1858, the Rev. Professor HAUGHTON draws attention to the opposite character of the specific actions of strychnine and nicotine on the muscular system. From the well-known facts that strychnine produces tetanic contraction of the muscles, whilst nicotine is a most powerful agent, whether administered in the form of tobacco-smoke or of infusion of the leaves, in relaxing muscular action, Mr. Haughton was led to believe that these poisons might be used as antidotes to each other, and with a view of testing the truth of this conjecture he made a series of experiments. These experiments were made by introducing frogs into separate solutions of nicotine and strychnine, and into similar solutions mixed together. He arrived at the conclusion that nicotine has the action of retarding, and in certain cases of completely counteracting, the effects of strychnine. In one experiment a frog had lived for forty-seven minutes in a mixture of two solutions, of which one would have destroyed life in four minutes; the other would have produced paralysis in one minute, and destroyed life in twenty-three minutes; and yet, in the mixture, the animal lived forty-seven minutes, and afterwards for twenty-four hours. In another experiment the frog, immersed in a similar mixture of the poisons for ten minutes, ultimately recovered, the effect of the strychnine being completely obviated by the action of the nicotine. Since the announcement of Mr. Haughton's results, Dr. O'Reilly, of St. Louis, Missouri, acting upon the suggestion, has successfully treated a case of poisoning by strychnine by the administration of infusion of tobacco leaves. The patient had taken six grains of strychnia; an emetic had been administered which had produced vomiting, but notwithstanding, the usual symptoms were fully developed. Dr. O'Reilly writes: "One hour and fifteen minutes after he had taken the poison we gave him the first dose of the tobacco infusion, which he swallowed with difficulty. We continued it in tablespoonful doses at intervals of five minutes, until he had taken half the quantity infused, before we had noticed a favourable change. Then the muscles became relaxed, the spasms less severe, and the intervals between them longer; and so conscious was the patient of relief, that he constantly called for the tobacco juice when he found the paroxysm approaching. This encouraged us to persevere with the infusion, prolonging the intervals between each dose as the frequency of the spasms abated; until finally, after twelve hours, they disappeared, leaving him in a state of fearful nervous prostration, from which he recovered in a few days under a careful tonic treatment. The whole quantity of tobacco used in making the infusion was one ounce and two drachms."—*B. and F. Med.-Chirurg. Rev.*, Oct., 1859.

49. *Arsenic, Fallacy of Sources of, in Dead Bodies.*—Dr. LOIS records the fact of his having made examinations of various specimens of brass. He discovered arsenic in ten specimens of brass, and in some specimens in large quantities. He considers this point as of importance to the medical jurist, inasmuch as various brazen ornaments—such as medals and rosaries, which are laid so frequently in coffins—may, by undergoing change in the presence of the products of decomposition, supply to the remains the poison which a chemist may be instructed to search for in the exhumations of persons who have been thought to die from poison.—*B. and F. Med.-Chirurg. Rev.*, Oct., 1859, from *Oesterr. Zeitschrift. f. pract. Heilk.*, No. xlix., 1859; and *Vierteljahrsschrift f. prakt. Heilk.*, No. xvi., 1859.

AMERICAN INTELLIGENCE.

DOMESTIC SUMMARY.

Trephining for Epilepsy and Insanity.—Dr. D. Brainard gives (*Chicago Med. Journ.*, Oct., 1859) brief notices of six cases in which he trephined for epilepsy and insanity. Though these were not related with sufficient details to be satisfactory, they are sufficiently interesting to be quoted:—

"This operation was performed on a man, thirty years of age, affected with epilepsy and some mental derangement for about four months. The point chosen was the left side of the occiput below the protuberance, the seat of a tumour which had existed for many years, and caused an absorption of the bone and a roughening of its surface. The two tables at the point of the operation were found consolidated and of an ivory hardness, and the skull of but half its natural thickness. The dura mater was vascular and adherent to the pia mater. The patient, who had been under the advice of Dr. Chambers, of Charleston, Coles County, Ill., returned home three weeks after the operation, very quiet, and much improved in his health. If kept from the exciting causes of insanity, there can be little doubt of his entire recovery.

"We have notes of five other cases in which this operation was performed for epilepsy, accompanied by more or less alteration of the mental faculties.

"The first of these was that of a young man, John Ladrigan, cut upon the head at two points by an axe in the forests of Wisconsin. One wound extended from the median line, at the junction of the coronal and sagittal sutures, to the left side three inches. The edge of the axe penetrated the substance of the brain deeply, portions of which escaped. The other cut, also on the left side, followed the line of junction of the parietal and occipital bones, and was as deep and as long as the other. At each a large piece of bone was partially separated, one edge passing upon the brain, and the other rising above the surface of the cranium.

"This man stated that he had been left as in a hopeless condition by the physicians who were called to see him, but recovered with the wounds in the condition we have mentioned, the right superior and inferior members being affected with paralysis and contraction. He was also subject to very frequent and severe paroxysms of epilepsy, which occurred sometimes daily.

"This man was operated on before the class of Rush Medical College.

"The whole of the displaced fragment of bone at the seat of the anterior wound was removed by three applications of the crown of the trephine. He recovered perfectly; had but one slight epileptic paroxysm afterwards, and gradually recovered considerable use of the members. No operation was performed on the posterior wound.

"The second case was that of a young man from Kendall County, Illinois. He received a blow above the ear, which fractured and depressed the skull without dividing the scalp. No operation was performed at the time, and he recovered, as was supposed. Six months afterwards he was seized with an epileptic fit. Three months afterwards another occurred, and then one every month. One year after the injury, we removed the depressed portion with the trephine. He recovered, and the epileptic paroxysms recurred at lengthening intervals, until, at the last notice, six months had elapsed without a return.

"The third case was that of a young man, kicked by a horse above the ear, fracture and depression being produced. Concussion and insensibility were the immediate results; after recovering from which, he remained insane, or nearly so. About four weeks after the injury, he was admitted into the wards of the so-called Mercy Hospital, where I used the trephine. Removing the depressed portions of bone gave immediate relief.

"The fourth case was a man of fifty years, who, eleven years previously, had a fracture with depression in the temporal region. This gave rise for several years to no perceptible inconvenience. At length he began to be affected with giddiness and loss of consciousness, which, by degrees, became more frequent, until the severer forms of epilepsy were developed. The mind was also affected by dementia to a degree which disqualified him for business. Removing the

depressed pieces of bone gave great relief, and one year after the operation he was steadily improving.

"The fifth case was not so favourable. A lad, about sixteen years old, came for advice in regard to severe epileptic attacks which had continued for several years. His friends stated that when an infant he had received a blow on the left superior part of the os frontis. On examining this part of the head, a depression was perceived, which, however, was soft and easily compressible. Although no bony surface could be felt at this point, yet the statement of friends of the patient that the blow had produced a depression, induced me to apply the trephine upon its margin. It was found, however, that the bone at that point was entirely absorbed; that surrounding it was in a condition of hypertrophy, very spongy and vascular, and three times its natural thickness. The soft parts presented this appearance in the point where the bone was deficient of venous erectile tissue.

"In this case no benefit appeared to result from the operation.

"From the four preceding, the inference is, I think, deducible, that leaving depressed pieces of cranium when there is neither wound of the scalp nor symptoms of compression of the brain, as is advised by most surgical writers, is attended by dangers not usually suspected, and that it is better, in all such cases, to raise up the depressed portions at once."

Vesico-Vaginal Fistula of Four Weeks' Standing.—Dr. E. P. SMITH, one of the Attending Surgeons of the Buffalo Hospital of the Sisters of Charity, records (*New York Monthly Review of Med. and Surg. Science*, August, 1859) the following case of vesico-vaginal fistula, successfully operated on by Dr. Sims' method:—

"Mrs. C., æt. 40, twice married, and has had ten children. With her last, was in labour forty-eight hours, brow presentation; head arrested and impacted. Ineffectual means were resorted to to change the position of the head; and perforation and extraction were had recourse to as a last resort. The delivery was effected March 12, 1859. Slight symptoms of hysteritis supervened, but yielded promptly to the usual remedies. Retention of urine existed for nearly a week, and the catheter was used daily. About ten days subsequent to the delivery of the fœtus, a detached slough of considerable size appeared, protruding through the vulva, and on being removed, was followed by a continuous flowing of urine, from the vagina. She entered the Buffalo Hospital of the Sisters of Charity, about the 1st of April, as a private patient. On making an examination, a vesico-vaginal fistula was at once detected, of about one inch in diameter, occupying the *bas fond* of the bladder, and situated about three and one-half inches from the vulva. The patient's general health is tolerably good, although she suffers greatly from the excoriated condition of the thighs and nates; she has also a large external hæmorrhoid. On April 21, 1859, I performed the operation so ably described by Dr. Marion Sims, in the *American Journal of Medical Sciences* for the month of January, 1852. The operation was somewhat tedious, and some slight trouble was experienced from the twisting of the silver wire used with the clamps, owing to its not being properly annealed. Three sutures were used, and the lead clamps were nearly two inches in length. The hemorrhage was trifling, and the patient complained but little during the operation. The self-sustaining catheter of Dr. Sims was then introduced, and the patient placed in bed.

"May 1st, I removed the clamps from the vagina, but continued the use of the catheter. During the ten days that have elapsed since the operation, she has been very comfortable, and, beyond a tympanitic condition of the bowels, which was relieved by injections, she has had no untoward symptoms whatever. About one week subsequent to the removal of the clamps, she left the hospital entirely well, being able to retain her urine as well as ever.

"June 25, 1859. She is now living with her husband, in the enjoyment of perfect health. In closing the report of this case, I cannot speak too highly of the instruments employed by Dr. Sims in this operation, than which nothing can be more simple and effectual; also of the position of the patient, and the use of the wide-guttered speculum, rendering an operation before so difficult of performance, one of the most easily executed in surgery."

Counter-Extension with Adhesive Plaster.—Dr. J. McF. Gaston, of Columbia, S. C., relates (*Charleston Med. Journal*, Sept., 1859) two interesting cases of fracture—one a comminuted fracture of the lower third of the femur, and the other a transverse fracture of the patella—successfully treated with adhesive plaster:—

Case I. "Rufus, a servant of Mr. John Davis, was placed under our care on June 16th, and upon a special examination with my partner, Dr. A. N. Talley, a comminuted fracture of the lower third of the shaft of the femur was discovered. With the assistance of two of our students, extension was effected, and after coaptation of the fragments, the roller bandage was applied to the entire limb. Three splints were next fitted to the thigh, and Welch's long extension apparatus was then adjusted by a gaiter, to secure the ankle to the foot-board beneath, and a silk handkerchief, wrapped around a roll of cotton as a perineal bandage, tied firmly to the upper end of the long splint above the crest of the ileum. By means of a screw connected with the apparatus, extension was now made, until the measurement from the anterior spinous process of the ileum to the internal malleolus was found to correspond to that of the sound limb.

"The muscular development of the thigh afforded great resistance, yet the extension was steadily maintained for a week, when there was such excoriation from the perineal bandage as to induce a discontinuance of this mode of counter-extension for the application of adhesive plaster, on the principle, but not precisely in the place, suggested by Dr. Gilbert, of Philadelphia.

"A strip of strong linen plaster, two inches wide, and two feet in length, was doubled upon itself at an angle, so as to make a loop, and yet both of the extremities having their adhering surfaces in the same direction. One of the free ends was pasted closely over the spinous process of the ileum, and extended down towards the inguinal region; while the other end was carried down and pasted over the trochanter, so that both pieces had firm attachments, by the *closeness* of the *integument* to *these bony prominences*, and thus presented resistance to traction upon the loop above. Transverse strips were placed from one piece to the other to secure their adhesion. A piece of homespun was now passed through this loop and made fast to the upper end of the long splint, thus effecting counter-extension. When the apparatus was tightened by the screw it was found that all the indications were met satisfactorily, while the excoriated hip and the perineum were left entirely free for such applications as the ulceration required. The strips adhered closely for three weeks without interference, and have proven an efficient means of counter-extension.

"Those who may be interested in this subject will find a well written article on the adaptation of adhesive plasters to counter-extension, by Dr. D. Gilbert, in the April number for this year of the *American Journal of Medical Sciences*, in which he details his mode of application and the principles involved in their use; while other articles from his pen may be found in the numbers for January of 1851 and 1858, in the same journal.

"The only particular in which my proceeding is entitled to any superiority over the process of Dr. Gilbert is, that it makes the attachment of the strips extend over the firm, unyielding points of bone, and thus serve, perhaps, to give a more fixed resistance to the force of extension. But while this is original with me, I prefer no claim which shall lessen the appreciation of the eminent service which has been rendered to the profession by the elaborate articles above referred to, and I trust that all may profit by their perusal, and cease to punish the subject of fracture with the excoriations of a perineal bandage."

Case II. "Being called on February 7th, of this year, to a servant of Mr. Robert Bryce, Dr. Talley and I went together, and found a transverse fracture of the patella, with the upper fragment drawn up considerably by the contraction of the muscles. Bringing the fragments in apposition, a figure of eight bandage was first applied, but subsequently, at the suggestion of my brother, Dr. J. B. Gaston, of Montgomery, Alabama, we resorted to the use of adhesive strips, to keep the fragments in apposition. Three pieces of plaster, an inch wide and twelve inches long, were carried from above the knee round the inner side of the patella to the upper and inner part of the leg; while three other similar pieces were carried from above the knee around the outer side of the patella to the upper and outer portions of the leg, thus keeping down the upper fragment of

bone securely. Another set of strips were attached in a semi-circular arrangement around the lower fragment, by which accurate apposition of the fragments was effectually maintained. This plan of treatment is attributed to Dr. Neill, and Erichsen makes reference to it in his work on Surgery. Our case progressed favourably, and though the union is not osseous, the connecting layer of cartilage is perhaps shorter and firmer than is usual in this accident, and does not interfere at all with locomotion.

"In a fracture of a similar kind, which I treated some years ago, the case was managed throughout by the figure of eight bandages, with a good result as to the union of the fragments; but I had to keep the entire leg and thigh encircled with a roller—the latter to control the action of the muscles, and the former to suppress the swelling which would have ensued from the obstruction to blood-vessels and absorbents at the knee. This difficulty is entirely obviated in using the adhesive plaster, as it extends only partially around the limb, and leaving that region free where most of the vessels are located. And the same views hold in reference to their application to the upper and outer part of the thigh, instead of extending round the inner part, where the vessels and nerves are located."

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New Method of Reducing Strangulated Hernia.—Dr. B. F. RICHARDSON, being called to a case of inguinal hernia, which resisted the usual methods of reduction, he was induced to adopt a new method.

"Reflecting," he says, "upon the mechanism in the *production* of hernia, I determined upon an expedient directly in opposition to the leading injunction of authorities. The patient was put upon his elbows and knees. Grasping the hernial tumour between my fingers and thumb, I pushed it steadily and firmly towards the inguinal ring; *he being at the same time directed to take a full inspiration and then make a strong and continuous expulsive effort, so as to distend the abdominal muscles as much as possible.* Between as well as during the expulsive efforts, the tumour was steadily pressed towards the ring. The reduction took place at the *second* effort. The time occupied was not over two minutes."

The *rationale* of the expedient he explains as follows:—

"The most usual cause of hernia is diaphragmatic pressure, induced through lifting, jumping, coughing, sneezing, blowing upon wind instruments, etc. Through the medium of the abdominal viscera the muscular parietes are distended and the apertures thereby enlarged, permitting the passage of intestine or omentum. The diaphragmatic force being diffused over the intra-abdominal surface, is easily antagonized by pressure at any particular point; and when attempting reduction by the manner proposed the diaphragmatic force should be more than counterbalanced, and the patient enjoined to permit the abdominal muscles to distend without restraint. Theoretically considered and practically confirmed, the conviction is irresistible, that this mode of management will reduce any abdominal hernia that is reducible without a surgical operation."—*Cincinnati Lancet and Observer*, Nov., 1859.

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Yellow Fever in New Orleans—its domestic origin.—We extract from an editorial in the New Orleans *Med. News and Hospital Gaz.* (Dec. 1859), the following remarks on this subject:—

"If there has heretofore been any doubt in the public mind as to the origin of yellow fever in our midst, it seems to us that the consideration of a very few of the facts connected with its late appearance must set every doubt at rest. While we have never uttered a syllable against the existing quarantine below our city—being, indeed, anxious to see the experiment thoroughly tested—we have never been able to bring our mind to the conclusion that yellow fever never originates here, but is absolutely imported. We have always thought that we observed facts enough to satisfy any unbiased mind of the capability of our city to originate the disease—indeed we have calmly looked upon what we were obliged to admit was its origin in our midst; yet we have carefully avoided entering into mixed controversies on the subject, believing that facts alone should be recorded as evidence. For the present season, then, we propose to place before the readers of our Journal, a very few facts—more than willing that each one should draw therefrom his own deductions.

First—We call attention to the fact, that the first death by yellow fever,

reported to the Board of Health this season, occurred on the 29th of September. This man came down the river from Vicksburg, and a note on the Board of Health report says that he was sick when he arrived. From all we can learn by personal inquiry, we entertain no doubt of the case having been yellow fever, but we give New Orleans the benefit of his having been sick when he arrived, and pass on to the case of Joseph Cooper, which, as will be seen, is entitled to precedence after all.

Dr. Hart, City Physician, has kindly furnished us with the data of this case, as follows:—

Joseph Cooper, native of Pittsburg, aged 17 years, arrived in New Orleans, from Pittsburg, on the 10th of September, on the steamer Camden. Having no means of procuring lodging, he was forced to sleep among the cotton bales on the levee the first night of his arrival. Early next morning he was found there by the police, and was carried to the "lock-up." That day (the day after his arrival) he was brought before the Recorder, and was sent to the workhouse as a vagrant. A day or two after he was seized with a chill, followed by high fever, and on the following day his symptoms were those of yellow fever. On the sixth day after his admission into the workhouse, he died with black vomit—the case presenting all the characteristics of genuine yellow fever. Dr. Hart has practised in this city a great number of years, and his acquaintance with this disease is too intimate to allow us to doubt his diagnosis. He gives us, too, the following account of other cases:—

Stephen Price, wife, two daughters, and one son, arrived from Cincinnati on the 25th of September. Some days thereafter, the eldest daughter was taken sick, and the following day the other daughter was taken. A priest, who was called in, gave them some patent pills. The next day Dr. Hart was called in, and lying on an old bed were the mother and two daughters, with clearly defined yellow fever. He commenced a course of simple treatment, but in a day or two he found they were all suffering so much for proper attention, that he advised the removal of the children to the hospital. The mother had the disease severely, but recovered sufficiently to sit up again. A few days after Dr. H. ceased visiting this woman, we were called one morning in a great hurry to see a patient said to be dying. On our arrival we found the woman dead. She had aborted a four months fœtus about three or four hours previously, and sank under the consequent hemorrhage. From the husband we now learned that this was Dr. Hart's case, and he also told us that one of his daughters had died at the hospital, and that the other was expected to die. This woman and family had gone from the steamboat on which they arrived, to the boarding-house of Duffy, on Girod Street, about midway between Tchoupitoulas and Levee, and here the mother and children were taken sick. This boarding-house is located in a true yellow fever region, according to all experience here. Certainly there is filth and impure air enough to originate any and all diseases.

We now give the notes of the first case that came under our own observation:—

"Mrs. M., young married woman—native of the Lake Shore, Mississippi—living on the New Basin, sent for us on the 14th of October. We found that she had been sick three days, and we have never in our life seen a more thoroughly marked case of yellow fever. We attended her during nine or ten days, and she had every grave symptom except black vomit. Her recovery was complete, but really unexpected. The patient had spent her first summer here, and had been remarkably careful in her habits. She had been down into the city shopping a few times during the summer, and went to church on Sundays, but spent far the greater part of her time in her room, sewing. She had been in contact with no shipping, and there were no other cases of fever in the neighbourhood.

"If the cases detailed did not take the fever as the result of causes originating here in our midst, will some sceptic furnish us the source whence they did derive it?"

Case of Abscess succeeding Childbirth.—Dr. Thos. B. Harvey relates (*Transactions of the Indiana State Medical Society*, May 17, 1859) the following interesting case of this: A woman, 23 years old, after a natural but protracted and very severe labour, gave birth to her second child, May 12, 1858. In con-

sequence of its adhesion, the placenta was artificially delivered. On the third day after delivery, the patient took a dose of sulphate of magnesia, upon the operation of which, in the evening, she was suddenly attacked with pain, which was found to arise from a prolapse of the uterus; it ceased immediately upon the replacement of the organ.

May 23, eleven days after delivery, the patient experienced pain in the left groin, extending thence down the inside of the thigh to the foot. Lochia arrested, tongue furred, skin hot, but perspiring. She was ordered calomel and Dover's powder, 10 grs. of each, followed, at the end of four hours, by a dose of sulphate of magnesia; a lotion of olive oil and laudanum to the affected limb, which was afterwards to be enveloped in cloths wet with hot vinegar. On the next day the pain in the groin and limb was relieved. The bowels had been moved; profuse perspiration; pulse 100. Fomentations to leg continued, drachm doses of sulphate of magnesia directed night and morning, with tincture of the veratrum viride.

On the 1st of June, symptoms of crural phlebitis relieved; tongue clean, appetite good; lochia resumed. All treatment, excepting a bandage to the leg, abandoned. Four days later the patient began to complain of a lancinating, but not continued, pain in the region of the left ovary, where a tumour could be felt, extending, just above the pubes, to the linea alba. It was about two inches in diameter, very hard, and although movable, it was apparently attached to the left ilium. Tongue furred, pulse 110, surface hot and bathed in perspiration, lochia entirely arrested. Stimulating liniments, and fomentations with hot vinegar, were applied locally, and pills of calomel, extracts of aconite and hyoscyamus, each one grain, with five drops of tincture of veratrum viride, given internally. This treatment was continued until the mouth became slightly affected, when the calomel was omitted.

June 23. The tumor had steadily increased in size, and now extended from the crest of the left ilium to the centre of the right hypogastric region, and to within an inch and a half of the umbilicus, the abdomen presenting an appearance similar to that of the fifth or sixth month of pregnancy. The tumour appears to be firmly attached to the left ilium, but at other parts is movable, allowing the finger to be passed under its right edge. A blister was applied over the tumour.

26*th*. The blistered surface freely suppurating—pain still experienced—tumour increasing in size; patient restless; pulse quick and feeble. The bowels heretofore were kept sufficiently open by small doses of salts, but have now to be assisted, on account of the pressure of the tumour upon the rectum, by occasional injections. Very little difficulty experienced in urination; urine normal in quantity. A rough, hard substance, similar to the knuckle of the partially closed hand, was felt on examining the tumour from the vagina. A mucous discharge takes place from the vagina whenever the patient is upon the chamber-vessel. Injections of warm water into the vagina were directed, to keep the parts clean, allay suffering, and favour the discharge of the tumour through the vaginal parietes.

July 2. No change in symptoms; blistered surface nearly well. The tumour distinctly pointing about an inch and a half above the pubic arch. Redness of the integuments at this part, to the extent of an inch and a half in diameter, indicates an attachment to that extent between these and the walls of the abscess. In consultation with Dr. R. C. Moore, it was decided to continue the same treatment, and watch the further progress of the case. On the next day, the pointing had taken a direction internally and upwards towards the umbilicus. Patient experienced very little rest, and had become quite feeble, the tumor presenting almost externally through the vagina, but at this part still hard; fluctuation very perceptible. Fearing that the internal wall of the abscess would yield to the pressure of its contents, and allow these to be discharged into the abdomen, an incision was made through the abdominal integuments, to the left of the median line, and an inch and a half above the pubic arch, into the abscess; a copious discharge of purulent matter followed, to the great relief of the patient. The discharge continued, more or less, for about two weeks; a hard, flat surface remained, which was gradually absorbed under the use of iodine. The patient was placed under a tonic and restorative treatment; she recovered rapidly, and is now (May 17, 1859) nursing a large, healthy child.

CHARLESTON PREPARATORY MEDICAL SCHOOL.

The Ninth Session of this School will begin at the Medical College of South Carolina, on the first Monday in April, 1860, and terminate on the 15th of July. The different Chairs will be occupied as follows:—

Institutes and Practice of Medicine,	D. J. Cain, M. D.
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Principles and Practice of Surgery,	Samuel Logan, M. D.
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Anatomy,	P. Gervais Robinson, M. D.

Clinical Instruction will be given at the Marine and Roper Hospitals, of which Drs. Porcher and Cain are Physicians. Drs. Lockwood and Bruns are respectively Physicians of the Poor for the Western and Eastern Divisions.

Among the patients of the Teachers, the students will have access to all cases to which they can with propriety be admitted.

When possible, Obstetrical cases will be shown to the students, by which means they will become acquainted with the practical details so essential to the successful management of such cases.

A complete Course on Operative Surgery will be delivered before the Class, and students will have an opportunity of performing the various operations upon the subject.

Recapitulations and examinations constitute an important feature in the course.

They beg leave to state also, that each department is illustrated by preparations, models, specimens, colored engravings, &c., to which additions are made from time to time, as the success of the School fully warrants. In short, every opportunity will be afforded for acquiring practical as well as theoretical knowledge of the profession. It may be added that the classes have steadily increased in numbers since the first establishment of the School.

Further particulars may be obtained by applying to any of the Teachers.

Students should have no fear of spending the early summer months in Charleston, as the city is remarkably healthy except when yellow fever prevails, which never commences before August or September.

Price of the Course, $50.

F. P. PORCHER, M. D., *Sec. and Treas.*

LONG ISLAND COLLEGE HOSPITAL, AT BROOKLYN, N. Y.

The first Course of Lectures in this Institution will commence on Thursday, March 29, 1860, and continue sixteen weeks.

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BROOKLYN, N. Y., *Dec.* 24, 1859.

HARVARD UNIVERSITY.

SUMMER SESSION OF THE MEDICAL DEPARTMENT.

THE Annual Course of Summer Instruction in the Medical Department of Harvard University will commence at the Massachusetts Medical College, in North Grove Street, Boston, on Monday, March 12, 1860, and continue till November.

Clinical Medical and Surgical Instruction will be given at the Massachusetts General Hospital, adjoining the College.

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During the Summer Session, instruction is given by lectures at Cambridge, on Botany, by Prof. Gray; on Comparative Anatomy, by Prof. Wyman; on Zoology, by Prof. Agassiz; on Acoustics and Optics, by Prof. Lovering. To these lectures Students of the Summer Session will be admitted without extra charge.

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Fees for the Summer Term (which must be paid in advance) $100, without extra charge for Matriculation, Hospital, Library, or Dissections; for six months, $100; for three months, $50.

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No. 132 *Tremont Street, Boston.*

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Jan., April—2t. January 1, 1860.

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Selections from a Report on Ovariotomy; read before the Kentucky State Medical Society, at its Annual Meeting, at Louisville, April, 1857. By J. Taylor Bradford, M. D. Augusta, Ky., 1859. (From the Author.)

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Introductory Lectures and Addresses on Medical Subjects, delivered chiefly before the Medical Classes of the University of Pennsylvania. By George B. Wood, M. D., LL. D., Pres. Am. Philos. Soc., &c. &c. &c. Philadelphia: J. B. Lippincott & Co., 1859. (From the Author.)

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The following Journals have been received in exchange:—

Moniteur des Sciences Médicales et Pharmaceutiques. Rédigé par M. H. de Castelnau. January, February, March, 1860.

Journal de Médecine de Bordeaux. Rédigé par M. Costes. November, December, 1859, January, 1860.

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The Medical Times and Gazette. January, February, March, 1860.

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The Dublin Hospital Gazette. January, February, 1860.

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The British American Journal. Edited by Archibald Hall, M. D. January, February, 1860.

Charleston Medical Journal and Review. Edited by J. Dickson Bruns, M. D. January, March, 1860.

The North American Medico-Chirurgical Review. Edited by S. D. Gross, M. D., T. G. Richardson, M. D., and S. W. Gross, M. D. Jan., March, 1860.

The New Orleans Medical and Surgical Journal. Edited by BENNET DOWLER, M. D. January, March, 1860.

The Boston Medical and Surgical Journal. Edited by F. E. OLIVER, M. D., and CALVIN ELLIS, M. D. January, February, March, 1860.

The New York Journal of Medicine. January, March, 1860.

The New Orleans Medical News and Hospital Gazette. Edited by D. W. BRICKELL, M. D., and E. D. FENNER, M. D. January, February, March, 1860.

The Virginia Medical Journal. Edited by J. B. MCCAW, M. D., and J. OTIS, M. D. November, December, 1859.

The Medical Journal of North Carolina. Edited by EDWARD WARREN, M. D. January, 1860.

Southern Medical and Surgical Journal. Edited by HENRY F. CAMPBELL, M. D., and ROBERT CAMPBELL, M. D. January, February, March, 1860.

New York Monthly Review of Medical and Surgical Science. Edited by AUSTIN FLINT, Jr., M. D. January, March, 1860.

The Maryland and Virginia Medical Journal. Edited by J. B. MCCAW, M. D., and W. C. VAN BIBBER, M. D. January, February, March, 1860.

The Chicago Medical Journal. Edited by D. BRAINARD, M. D., and E. INGALS, M. D. January, February, 1860.

The Ohio Medical and Surgical Journal. Edited by JOHN DAWSON, M. D., and J. M. HAMILTON, M. D. January, 1860.

The Medical and Surgical Reporter. Edited by S. W. BUTLER, M. D., and R. J. LEVIS, M. D. January, February, March, 1860.

The Cincinnati Lancet and Observer. Edited by E. B. STEVENS, M. D., and J. A. MURPHY, M. D. January, February, March, 1860.

St. Louis Medical and Surgical Journal. Edited by M. L. LINTON, M. D., and W. M. MCPHEETERS, M. D. January, 1860.

The Pacific Medical and Surgical Journal. Edited by J. B. TRASK, M. D., and D. WOOSTER, M. D. December, 1859, January, 1860.

Louisville Medical Journal. Edited by THOMAS W. COLESCOTT, M. D. Feb., March, 1860.

The Cincinnati Medical and Surgical News. Edited by A. H. BAKER, M. D. January, February, 1860.

The Savannah Journal of Medicine. Edited by JURIAH HARRISS, M. D., and R. D. Arnold, M. D. January, 1860.

The American Journal of Insanity. Edited by the Medical Officers of the New York State Lunatic Asylum. January, 1860.

American Journal of Pharmacy. Edited by WM. PROCTER, Jr. January, March, 1860.

Nashville Journal of Medicine and Surgery. Edited by Drs. W. K. BOWLING, R. C. FOSTER, and GEO. S. BLACKIE. January, February, March, 1860.

The Saint Joseph Journal of Medicine and Surgery. Edited by Drs. HEDDENS, CHAMBERS, and SCEARCE. January, March, 1860.

The New York Medical Press. Edited by Drs. J. L. KIERNAN and W. O'MEAGHER. January, February, March, 1860.

The American Medical Monthly. Edited by J. H. DOUGLAS, M. D. January, March, 1860.

Atlanta Medical and Surgical Journal. Edited by JOS. P. LOGAN, M. D., and W. F. WESTMORELAND, M. D. Nov., Dec., 1859, Jan., Feb., 1860.

Cleveland Medical Gazette. Edited by G. C. E. WEBER, M. D. Dec., 1859, January, 1860.

The Journal of Materia Medica. Edited by JOS. BATES, M. D., and H. A. TILDEN. January, February, March, 1860.

The Belmont Medical Journal. Edited by WM. ESTEP, M. D., and E. GASTON, M. D. November, December, 1859, January, February, 1860.

The Peninsular and Independent Medical Journal. Edited by A. B. PALMER, M. D., MOSES GUNN, M. D., and F. STEARNS. January, February, 1860.

The Kansas City Medical and Surgical Review. Edited by G. M. B. MAUGHS, M. D., and T. S. CASE, M. D. January, 1860.

The Dental Cosmos. Edited by J. D. WHITE, M. D., J. H. MCQUILLEN, D. D. S., and GEO. J. ZIEGLER, M. D. January, February, March, 1860.

Oglethorpe Medical and Surgical Journal. Edited by H. L. Byrd, M. D., and Wm. Hauser, M. D. January, 1860.

The North American Medical Reporter. Edited by W. Elmer, M. D., and Louis Elsberg, M. D. August, November, 1859.

The Chicago Medical Examiner. Edited by N. S. Davis, M. D., and E. A. Steele, M. D. January, February, 1860.

The American Journal of Science and the Arts. Edited by Profs. B. Silliman, B. Silliman, Jr., and J. D. Dana. January, March, 1860.

The Nashville Medical Record. Edited by Drs. D. F. Wright, T. L. Maddin, and J. H. Callender. January, February, March, 1860.

The American Journal of Dental Science. Edited by Chapin A. Harris, M. D., and A. Snowden Piggot, M. D. January, 1860.

The Dental Register of the West. Edited by J. Taft and Geo. Watt. Jan., February, March, 1860.

Louisville Monthly Medical News. Edited by S. M. Bemis, M. D., and J. W. Benson, M. D. January, February, 1860.

American Druggists' Circular, and Chemical Gazette. January, February, March, 1860.

The Druggist—A Monthly Newspaper for the Trade. January, February, March, 1860.

The San Francisco Medical Press. Edited by E. S. Cooper, M. D. January, 1860.

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